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THE PSYCHOLOGICAL REVIEW

THE CONCEPTUAL CATEGORIES OF PSYCHOLOGY: A SUGGESTION FOR REVISION [1]

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The psychologist who wishes to propose a revision of the traditional concepts of his science need not rely solely upon his own opinion as sanction for the enterprise. He is encouraged by his colleagues in both precept and example. Within the last few years, presidential addresses delivered before the American Psychological Association and its affiliated societies have frequently criticized the conceptual categories of psychology and proposed revision from one or another point of view. Selecting almost at random from among these addresses, I find such remarks as the following:

> There has never been a period in the history of psychology in which the experimental contributions to that science have been so numerous as at the present time, or more significant. And yet this productivity is accompanied by an evident uncertainty concerning the subject matter and the fundamental categories of the science (17, p. 1).

> Any modern psychology text is an eloquent witness to what a jumble of loosely related and often incompatible concepts have resulted from our habit of relying on many diverse experimental approaches for their definition (1, p. 390).

[1] For criticism of the manuscript I am grateful to my colleagues at the Woman's College, Professors J. A. Highsmith and W. W. Martin; to Professor J. F. Dashiell of the University of North Carolina; and to Professor D. K. Adams of Duke University. They should not, however, be held responsible for the ideas expressed here.

Parts of this paper were read before the Psychology Section of the American Association for the Advancement of Science in December 1939 and before the American Psychological Association in September 1940.

Recent years have witnessed a growing dissatisfaction on the part of psychologists with the systematic analysis of psychological events represented by the traditional categories (17, p. 227).

Psychology is unprogressive today less for the lack of a great man as suggested by Boring than for lack of adequate conceptual tools (11, p. 500).

There is some measure of agreement that the origin of psychology's conceptual difficulties is to be found in part in the fact that the subject is young as a science but old as a topic of investigation by general observational and speculative procedures. These procedures have resulted in the formulation of a large number of loose descriptive categories, which have gained general recognition as the terms in which human behavior is to be understood. The words representing these categories have become so firmly embedded in our language that few laymen, and indeed not all psychologists, have been aware that they represent merely logical constructs and not existential entities. The universality of the *word* has been taken as evidence for the existence of the *thing*. Even where this reification has not occurred, the word has still imposed upon us a particular logical division of phenomena which has become so habitual that it is now difficult for us to conceive of other, perhaps more useful, divisions which would cut across our present categories. The psychologist has, in most instances, accepted the categories of the layman and attempted merely to find various correlates for them, or to give them more objective definition.

The investigation of the category 'emotion' furnishes an excellent example of this procedure. Few psychologists have questioned whether this particular logical construct, or division of phenomena, is a useful one. On the contrary, investigators of the 'emotions' have accepted the division of phenomena without question and have attempted to find, for example, physiological correlates of emotion. When their experimental attacks upon the problem yielded little but confusion, they still failed to question the category itself. They looked merely for better experimental methods. For example, when it was found that the galvanic skin response failed to provide an index of, or to correlate highly with, emotion or any other of the psychological categories, psychologists with one accord be-

came critical of the value of the psychogalvanic reflex rather than critical of the value of the category 'emotion,' or that of certain other established divisions of psychological phenomena. The galvanic skin response was very nearly discarded as being of no psychological interest. However, it was obvious that skin resistance showed consistent variations in the same individual under different physiological and psychological conditions. For instance, it was different in the sleeping and the waking states; it was different during excitement and calm, during tension and relaxation. Whether or not it correlated with any established psychological categories, its variations seemed worthy of study. Recently it has been suggested that it may be an indicator of a new psychological category which we have been forced to set up, *i.e.*, the general reactivity, or energy level, of the individual (**4, 5, 13**).

The phenomena of nature do not come to us ready-organized except in limited degree. On the contrary, we impose upon these phenomena our own organization. It is of considerable importance *which* distinctions we make, which categories we establish. Many are possible, but few are relevant for a given purpose. For example, it is a commonplace that we may distinguish between substances on the basis of their color, but that for the purposes of chemistry this is a less relevant distinction than one made on the basis of molecular composition, a classification which disregards the color classification. We know also that we may speak of the falling of an apple, the oscillation of a pendulum, and the course of the planets as separate and distinguishable phenomena, as indeed they are, but that for certain purposes of physics it will be more useful to disregard the differences between them and view them all as manifestations of the operation of the Newtonian law of gravitation.[2] Similarly, certain divisions of psychological phenomena, as represented by the traditional categories, are *possible*, if we content ourselves with rough distinctions, but they may not be *useful* in psychological explanation. Other classifications, which would cut across the present conceptual categories, might well prove to

[2] I am indebted to Lewin (20) for certain features of this interpretation.

be simpler and more satisfactory. Many of the present concepts of psychology are vague and overlapping. Moreover, they frequently imply dichotomies where the phenomena in fact show continuous variation.

It is hardly necessary to point out that a major requirement of a satisfactory system of concepts is that the concepts shall be as few and as fundamental as possible, in order that the maximum number of phenomena may be included under the minimum number of explanatory principles. It is my purpose to show that *there can be found among the phenomena described by our present psychological categories underlying unities which would enable us to classify these phenomena into fewer and more inclusive divisions.* Such divisions would not only simplify psychological description, but would also prevent our looking for different correlates of similar phenomena which, through historical or linguistic accident, have been placed in different conceptual categories.

An examination of psychological concepts reveals immediately that they fall into two roughly distinguishable types: (1) *longitudinal* concepts, or concepts describing phenomena which occur in *sequence*, and (2) *cross-sectional* concepts, or concepts describing phenomena which occur *simultaneously*. Examples of longitudinal concepts are those of 'stimulation' and 'response,' and those of 'maturation' and 'learning.' These phenomena occur in irreversible sequence. There is progress from one point to another. By means of these concepts we describe the forward direction of behavior. Examples of cross-sectional concepts are those of 'attention,' 'perception,' 'thought,' and 'emotion.' These concepts describe simultaneously occurring phenomena, or different aspects of the organism's behavior at a given moment.

The longitudinal concepts of psychology have recently undergone considerable discussion and revision. For example, the S–R, or stimulus-response, unit has yielded place to a unit of broader scope and different emphasis, *e.g.*, the 'behavior-act' of Tolman (**28**) or the 'start-to-end' unit of Muenzinger (**26**). Lewin (**21, 22**) has offered a new conceptual framework for psychology, and Hull (**15, 16**) has presented a

'scientific theoretical system in miniature' which may be applied to the problems of learning. These significant contributions to psychological theory do not, however, include a revision of that group of psychological concepts which I have referred to as 'cross-sectional,' though they might in certain instances be extended in such a way as to perform this function. The cross-sectional concepts have received little critical attention and are still employed in essentially the same form in which they were employed by the faculty psychologists. Though our textbooks protest that such concepts as 'attention,' 'perception,' 'thought,' and 'emotion' now represent for us, not *entities* or *faculties*, but *activities*, these 'activities' appear to be regarded, not as convenient and somewhat arbitrary divisions of an essentially unitary phenomenon, the response of the organism, but as ultimate divisions of qualitatively distinct phenomena, which may actually follow different principles of action. It is the purpose of the present paper to offer a criticism of the cross-sectional conceptual categories of psychology and to propose an alternative group of concepts which are simpler and more inclusive. The concepts to be proposed do not represent inventions of the writer. Instead, they are a familiar part of current psychological description. They are concepts which exist side by side with the concepts handed down from 'faculty' psychology. They have developed as virtually necessary logical constructs derived from accumulated empirical data. Psychologists have failed, however, to recognize their wide applicability. They have failed to recognize that, by making use of the new concepts, they could dispense with the old. Instead, they have held to the old concepts as primary and have employed the new ones as subheads of explanation. This procedure has resulted in great confusion, one manifestation of which has been the formulation of experimental problems in misleading conceptual terms.

An adequate discussion of the cross-sectional units of description which I propose demands that these concepts first be placed in their setting in the subject matter of psychology. I shall digress, then, to define the subject matter of psychology

and the longitudinal unit of description which appears to be most useful.

Psychology deals with the adjustive activities of the organism, or those responses by means of which the organism maintains its internal equilibrium in the face of threatened disruption from internal and external sources. If it be objected that physiology also deals with these activities, it could be said that no sharp distinction is possible between physiological and psychological phenomena, though the phenomena called psychological may be said to be found chiefly at that end of the continuum of response where mnemic phenomena occur to the most marked degree.[3] Since all activity, both physiological and psychological, appears to be *adjustive*, *i.e.*, *goal-directed*, a convenient longitudinal unit for psychological description will be found in that segment of behavior which begins with a particular maladjustment or disequilibrium, in the form of some disturbing stimulating condition, and ends with the final response which the organism makes in the attempt to restore equilibrium or remove the particular disturbing conditions. As Muenzinger has expressed it, the most convenient unit of psychological description appears to be a cycle of behavior which is "characterized by a constant direction, *i.e.*, a persistent direction towards the same end-phase" (**26**, p. 11). Such cycles of behavior do not necessarily terminate in satisfactory adjustment before they are interrupted by other cycles of behavior directed toward other end-phases. However, the unit of description includes all the responses directed toward a particular goal. Since some goals are immediate and others remote, and since some goals are subsidiary to the attainment of other goals, our description may involve a hierarchy of units.[4]

Several points may be noted in connection with the unit of description here proposed. First, it gives primacy to the needs of the organism, and not to the conditions of the environ-

[3] For this interpretation I am indebted to the discussions of Bertrand Russell (**27**) in *The analysis of mind* and of H. L. Hollingworth (**14**) in *The psychology of thought*.

[4] The interpretation in this paragraph has been influenced by Tolman (**28**) and by Muenzinger (**26**). I am indebted to them also for certain phrases used here and elsewhere in the paper.

ment, as the starting point for behavior. Secondly, it locates the beginning of behavior in stimulation and its termination in response, but the stimulus-response unit which is employed differs from the traditional behaviorist S–R unit in two respects: (1) the initiating stimulating condition, representing a need of the organism, is within the organism, not in the external environment; (2) the stimulus-response unit is more inclusive, since it comprises all the responses directed toward a given goal or end-result.

If our longitudinal unit of description is determined by reference to a goal, it appears reasonable, in searching for cross-sectional units of description, to inquire *what are the characteristics or aspects in which a given cycle of response may show variations which affect the adequacy of the response unit as an adjustment, or determine the degree of success of the behavior in reaching its goal.* We may ask, then: In what respects does the behavior set up by a given stimulating condition, and directed toward a given goal, show variations which are significant in determining the quality of the response as an adjustment? If our answer is given in terms which are broadly inclusive, if our conceptual categories are as few and as fundamental as the present state of knowledge makes possible, we shall have identified, for present purposes, the *functional units of response*, or the dynamic variables intervening between the initial and final phases of any response.

The question which I have raised is, logically, composed of two questions: (1) In what respects do *cycles* of response show variation? (2) In what respects does any given *response* within the cycle show variation? The answer to the first question will, however, constitute an answer to the second, since any response within the cycle may be described in the same terms as the cycle as a whole. However, the characteristics attributed to individual responses derive their meaning from the fact that the individual responses are part of a larger whole and can, therefore, be described in terms which are significant for that whole but might be without meaning as applied to a single response which was not regarded as a part of such a whole. In other words, the functionally significant

characteristics of the individual responses are cross-sectional views of the characteristics of the response cycle.

There are, so far as I can discover, three, and *only* three qualities which have functional significance and are common to all responses: (1) direction, (2) response to relationships, and (3) energy level. Any response, overt or implicit, manifests direction toward a goal, involves response to relationships, and occurs at some particular energy level. Variations in these three aspects of the response determine variations in the effectiveness of the response as an adjustment. It appears that all functionally significant qualities of responses can be adequately described in these terms, and that we can, through the use of these concepts, dispense entirely with the traditional cross-sectional concepts of faculty psychology. Attention, perception, thought, and emotion, for example, represent loose and overlapping categories of phenomena, *not* functional units of response. The phenomena described by these terms can, I believe, be more simply and adequately described in the terms which I have proposed above, and when we have so described them we add nothing to the understanding of them by employing, in addition, the traditional terms with which we are so familiar. These latter terms may be useful for communicating with the layman, but they are not useful as scientific concepts.

To recapitulate briefly, response originates in a stimulating condition which represents some form of disequilibrium of the organism. It is directed toward the attainment of better equilibrium. To achieve this result the organism (1) shows selectivity of response, or direction in its behavior, (2) observes relationships in the external situation and in the situations implicitly represented, and (3) mobilizes energy as the occasion demands. Variations in the quality of these three aspects of the response determine variations in the quality of the behavior as an adjustment. Behavior which we call stupid or abnormal, or otherwise inadequately adjustive, is behavior which is inadequate in one or more of these aspects. It appears, then, that these are the only concepts required for the description of the functional variables in response, though

additional concepts are required for a description of the stimulating conditions giving rise to activity and, perhaps, certain characteristics of the completed response. However, the functional variables of any response, overt or implicit, perceptual, thought, or emotional, appear to be completely described by the three concepts outlined above.

As I have pointed out, no one of the three concepts is basically new. All of them are in more or less general use. They came into being because they were needed for psychological explanation. They are the result of a large body of experimental investigation. They do not represent merely *a priori* analysis. They are concepts almost unknown to the layman, but quite familiar to all psychologists. Wider use of these concepts, I shall attempt to show, would enable us to dispense with a number of the traditional categories into which psychological phenomena have been divided. For many years concepts based upon functional unities in response, and representing distinguishable aspects of the total behavior of an adjusting organism, rather than hard and fast divisions between separate *kinds* of behavior, have existed side by side with concepts handed down from 'faculty' psychology. The substitution of these concepts for certain of our traditional concepts would, I believe, simplify the organization of psychological data and lead to more adequate formulation of problems for experimental investigation.

Examining in more detail the concepts which I propose as fundamental in the description of the dynamic variables of response, we note that the first concept, that of the *direction* of response, derives from the fact that every cycle of response shows the characteristic of direction toward a goal.[5] Moreover, response cycles vary in the degree to which goal-direction is maintained throughout the cycle, and any given response may be described as either more or less in line with the general goal-direction. The adequacy with which direction is maintained during a behavior cycle is one of the factors determining the quality of the behavior as an adjustment.

[5] 'Direction of response' refers, of course, to *psychological* and not to *physical* direction.

Maintenance of direction involves selectivity of response. The appearance of certain forms of response is facilitated and the appearance of other forms of response is inhibited. For example, the stimulating condition of hunger initiates a cycle of behavior directed toward the goal of removing the disequilibrium represented by the conditions giving rise to the hunger stimulation. Under these conditions the appearance of restless, exploratory responses is facilitated, while the appearance of relaxed behavior is inhibited. If previous experiences have enabled the animal to build up expectations of the means by which his hunger may be satisfied, the stimulating condition of hunger in a particular kind of environment will cause the selection in considerably greater detail of the kinds of responses which will occur.

The maintenance of direction in behavior may be observed in both overt and implicit responses. An individual attempting to reach a goal may move in a certain physical direction, or he may manipulate certain objects, and not others. At the same time it is apparent that his implicit responses also are showing selectivity. He notices or 'pays attention to,' not all aspects of his environment, but those particular aspects which are in line with his goal. His thought processes show the influence of what has been called a 'determining tendency.' They are directed toward the goal set by the problem.

The psychological concepts of 'set,' 'attention,' 'determining tendency,' and 'motive' all refer to the phenomena of direction in behavior. These phenomena are essentially the same whether they occur in overt or implicit behavior, and whether they are stimulated by tissue conditions or other means. The fundamental psychological principle is that responses are *selected* in terms of a more or less consistently maintained direction in behavior, which can be defined by reference to a goal, and which, like all other aspects of response, represents an adjustment to the present internal and external stimulating situation.

If the maintenance of *direction* is a basic aspect of all behavior, a functional unit of response, then variations in the maintenance of direction should result in variations in the

adequacy of behavior as an adjustment. Such is found to be the case. Inadequate maintenance of direction in response results in various forms of imperfectly adjustive behavior. We describe the individual manifesting such behavior as being unco-ordinated in his responses, or as being impulsive, or distractible, or unable to concentrate. Adequate maintenance of direction appears to be characterized by flexibility rather than rigidity of behavior, so that, while the general direction of response is maintained, there is variability in the specific details of behavior, rather than the rigid maintenance of a given form of response, such as is seen in 'perseveration' or in stereotyped behavior. The rat in the maze, for example, maintains his orientation toward the food box, but he traverses, not always the same, but many equivalent routes in approaching his goal (6). Loss of flexibility appears to be characteristic of certain types of mental disorder.

The ability to maintain direction, or to inhibit and co-ordinate responses, is only slightly developed in early childhood. It is this fact which accounts for much of the inadequacy of the child's behavior. The child is impulsive. He manifests uninhibited emotional outbursts. He 'jumps to conclusions' in his thinking. He enters a large number of blind alleys in the attempt to find his way through a maze. He shows lack of ability to resist distractions, and hence to concentrate on any given task for a long period of time. It is difficult for him to carry out instructions to make a movement with maximum slowness (23, p. 344). These characteristics of the child's behavior represent diverse manifestations of deficient development of a single aspect of response, the maintenance of direction. For it is apparent that direction can be maintained, within the limits of the individual's comprehension of relationships, only when there is the ability to *inhibit* certain responses so that other responses may have the right of way. The child 'jumps to conclusions' not only because a limited experience sometimes fails to present alternative possibilities, but also because he is not able to restrain the forward movement of a given possible solution to a problem until it can be compared with other solutions. Suspended judgment

involves inhibitory ability. Weighing the evidence and reaching a correct conclusion represents, in the realm of implicit responses, the same phenomenon which is observable in overt responses when the individual restrains useless movements in such a way as to give a skillful, well co-ordinated performance. Both represent the maintenance of direction in behavior.

In this aspect of response, as in the other two aspects which I propose as fundamental, we find not only age differences but also individual differences. In reaction time experiments it has been observed that there are differences between individuals in tendency to 'jump the gun.' There are differences, which have been called differences in 'temperament,' in the type of performance which individuals give on maze tests. There are also differences between individuals in their ability to hold to their purposes and pursue a consistent line of behavior over a long period of time in the face of diverting influences. Direction in behavior is a significant aspect of responses classified in many different categories.

The second concept which I propose as fundamental in the description of the dynamic variables of response is that of *response to relationships*. If the individual is to reach his goal, he must respond to certain relationships in the situation. His response can be appropriate only if it represents adequate discrimination of those relationships which are relevant to his purposes. This discrimination need not be conscious, or to speak more accurately, the individual may respond appropriately to relationships of which he could not give a verbal report, and which he may not be aware of responding to as such.[6] Response cycles vary in the degree to which they represent adequate response to relationships, and any given

[6] Adjustment, involving response to relationships, appears to be the fundamental process. The degree of awareness of the relationships responded to may vary from no awareness at all, or almost no awareness, to full awareness of every detail. Most responses occur with an intermediate degree of awareness of the relationships to which response is made. The process of adjustment appears to be fundamentally the same whether the individual is conscious or unconscious of the relationships to which he responds. No doubt, fully conscious adjustments do differ in certain respects from less conscious adjustments. They may, for example, make it possible to bring a wider group of considerations to bear upon the situation. It is my belief, however, that the primary features of the adjustive process remain the same throughout the continuum of consciousness-unconsciousness.

response within the cycle may be said to be either more or less adequate from this point of view. The quality of the behavior as an adjustment is determined in part by the adequacy of this aspect of response.

By 'response to relationships' I refer to the fact that stimuli have *meaning* for the individual. This meaning is, of course, a function of their relationship to the goal of a particular response cycle; the same stimuli would have different meanings in different cycles of behavior. They might also have different meanings if they occurred in conjunction with a different set of surrounding stimuli. And their meaning would be different if the past experience of the responding individual had been different, for it is on the basis of his past experience that the individual arrives at a tentative interpretation of, or meaning for, the present stimulus situation. We may say, then, that the behavior of the individual is directed toward present stimuli, not as discrete and independent entities, but as *related to each other*, as *related to the individual's past experiences*, and, above all, as *related to the individual's present goal.*

It is obvious that both overt and implicit responses show response to relationships. The individual's movements in space, his manipulation of objects, his perception of his surroundings, and his thinking about his problems all manifest response to relationships.

Psychological concepts which emphasize this phenomenon are those of perception and thought. The fundamental characteristic of both perceiving and thinking is that of response to relationships, and the distinctions which we attempt to draw between what we designate as two separate processes are artificial and misleading. Perception is described as a response to relationships found among sensory stimuli, while thinking is described as a response to relationships among symbolic stimuli. It is well recognized, however, that most, if not all, perceptions involve thinking, since only a few of the relationships responded to in a perception are relationships among the sensory stimuli experienced at the moment. Many of the relationships are symbolic in the sense that they are relation-

ships between a given sensory datum and some experience in the past which it stands for or is otherwise connected with. On the other hand, thinking is often carried on with reference to objects which are present to the senses. *Response to relationships*, as a concept, has the advantage of emphasizing the underlying unity of the phenomena of perceiving and thinking, and suggesting a continuum of processes rather than a dichotomy.

Variations in the degree of discrimination of relevant relationships determine variations in the effectiveness of responses as adjustments. Many responses are ineffective because they represent only a partial discrimination of the relevant relationships. Some important factor in the situation has been overlooked. For example, in the 'detour' experiments certain animals attempt to reach their goal by going directly toward it instead of taking account of the fact that the direct path is blocked while a more round-about path is open. Other responses are ineffective because they represent a persistent, stereotyped distortion of relationships. For example, the responses of the paranoic represent an interpretation of the situation which is determined by a persistent persecutory trend which gives the *direction* to behavior and determines the relationships which shall be responded to. Though continued experience with the situation might be expected to reveal to the individual the inadequacy of his interpretative organization, he continues to respond in terms of a fixed pattern of relationships. Explanation of this fact must, of course, be sought in the adjustive function served by the pattern of relationships which is fixated.

The ability to respond to relationships varies with the maturity of the individual, with the individual's experience with the particular relationships in question, and with certain factors of constitution which differentiate one individual from another or one species from another. It is a major factor in the type of performance measured by the intelligence tests. In early childhood this ability is only slightly developed, since both maturity and experience are limited. Many of the im-

perfectly adjustive responses of the child are due to imperfect response to relationships.

The third concept which I propose as fundamental in the description of the dynamic variables of response is that of the *energy level* of response. Response cycles, and the separate responses within a cycle, vary in the organic background of energy at which the activity occurs. Responses which have the same direction of behavior and the same response to relationships may occur at different energy levels, and the particular energy level at which the response occurs will determine in certain important respects the character of the response. For example, an individual walking toward his home will maintain a definite direction in his behavior and will respond to the relationship of one street to another, yet his walking behavior will be different according to whether he is tired or well rested. It will occur at a different energy level. Or the tired individual, walking slowly toward home with a minimum expenditure of energy, may notice smoke coming from the direction of his house. His behavior will suddenly change. He will maintain the same general direction of response, since the goal of his behavior is still that of reaching home (though now he desires to reach home as quickly as possible), and he will respond to the same relationships among the streets, but now he will move quickly and energetically, and he will have lost all sense of fatigue. His energy level has been raised in response to the stimulus of thinking that his home is on fire. This change in energy level is adaptive to the demands of the situation, as was the low energy level at which he was formerly proceeding. In the absence of an emergency it is desirable for the fatigued individual to expend little energy. In the presence of an emergency his behavior must be vigorous, even at the cost of depletion of energy reserves. Energy level, like the other two aspects of response which I have discussed, represents an adjustment to the situation in which the individual finds himself, and the appropriateness of the energy level of response is one of the factors determining the quality of the behavior as an adjustment.

By the term 'energy level,' or 'level of reactivity,' I refer to the phenomena of mobilization of energy which Cannon (2) found to occur in extreme degree during the excited emotions; to what, in an early publication (8), I called 'degree of excitation' and, in a later publication (10, p. 194), I defined as "the extent to which the organism as a whole is activated or aroused, not as indicated by overt behavior, but as measured by the activity of those processes which supply the energy for overt behavior"; and to what Freeman (13, p. 326) has more recently called the 'postural substrate,' or "the general organic background (neuro-glandular-muscular) which operates to sustain and energize overt phasic responses."

The effect of energy level upon behavior may be appreciated more fully if we contrast the responses of the sleepy or relaxed individual with those of the alert, highly excited, or tense individual. In the former case responses are feeble, in the latter they are forceful; in the former instance the reaction time is long, in the latter it is short; in the former instance stimulus thresholds are high, in the latter they are low. The excited individual is alert and forceful; the sleepy or over-relaxed individual is dull and inert. Reactivity level, or energy level, varies in a continuum from a very low level, such as is found in deep sleep, to a very high level, such as is found during extreme excitement or agitation, or during supreme physical effort.

It seems probable that energy level is a determining factor in variations in co-ordination, or organization, of responses. An energy level which is very high or very low frequently results in disorganized behavior. Disorganization of response accompanying a high energy level may be seen in the behavior of the too highly motivated individual who must relax before he can perform effectively. Disorganization of response accompanying a low energy level may be seen in the behavior of the fatigued or the drowsy individual.[7]

The effect of energy level upon the co-ordination of responses appears to be a function, however, not of the energy

[7] The 'fatigue' referred to here is that in which a low energy level is found. It is possible that there are conditions properly defined as 'fatigue' in which the energy level is high.

level as such, but of the relation between energy level and the individual's ability to maintain direction in his behavior (9). Where this ability is highly developed, co-ordination of responses may be maintained even at an energy level which is ordinarily hazardous to effective control. Individuals differ greatly in the adequacy of the *directional* aspect of their responses. Behavior occurring at an energy level which would be disruptive for some individuals may, in the case of other individuals, proceed in smooth and well co-ordinated fashion. Moreover, the same individual probably differs from time to time in the adequacy of this aspect of his adjustments.

Energy level is a no less significant determiner of the characteristics of *implicit* behavior than of those of *overt* behavior. Just as in overt behavior changes in energy level are accompanied by changes in the speed, intensity, and co-ordination of responses, so in implicit behavior similar phenomena are to be observed. Thought processes are slow in fatigue, rapid in excitement; experiences are vivid when the individual is alert, but of low intensity when he is drowsy or fatigued; co-ordination of thought processes is frequently impaired during fatigue or excitement, but is attained to a high degree at a moderately high energy level.

The effect upon behavior of changes in energy level appears to be independent of the *kind of stimulation* from which the change in energy level results. It may be suggested, pending further investigation, that even the effects of drugs which act as excitants or depressants are, if no auxiliary physiological effects are involved, the same as the effects of any other stimuli which result in the same energy level. Some psychologists give the name 'emotion' to very high or very low levels of energy (excitement and depression) which are brought about, not by routine physiological adjustments, but by the individual's interpretation of a certain stimulus situation as having marked significance (favorable or unfavorable) for the attainment of his goals. It has not yet been demonstrated, however, that energy levels resulting from this type of stimulus differ in their effect upon behavior from energy levels resulting from any other type of stimulus, provided the particular level of energy is the same in both cases. It is true, however, that

the most marked changes in energy level are usually the result of the kind of stimulation which is called 'emotional,' for stimulation of this sort is by definition the kind received from situations which are of the gravest concern to the individual. A further characteristic of so-called emotional responses, *i.e.*, that they are usually disorganized, derives from the fact that disorganization, or inco-ordination, of responses is a frequent, though not a necessary, accompaniment of extremely high or extremely low levels of energy. But the primary phenomenon is that of the energy level of the individual's responses. Other behavior phenomena are derivative from this and from the two other primary aspects of response, maintenance of direction and response to relationships. The nature of the stimulating situation is important as a determiner of the direction of behavior, of the relationships which will be responded to, and of the energy level of response. It does not, however, determine the occurrence of a particular *kind* of behavior, 'emotion,' which because of its own peculiar nature results in a characteristic pattern of response.

Changes in energy level are assumed to occur, not only as a part of 'emotion,' but also as a part of motivation. A motive, like any other activity, originates in a certain stimulus situation which represents a disequilibrium of the organism. The phenomena of motivation are said to be (1) the maintenance of direction in behavior and (2) an increase in energy level. These phenomena are similar to those described under the category emotion, though emotional behavior is said to be characterized at times by a *decrease* rather than an *increase* in energy level, and emotion is said to involve a distinctive (undefined) feeling tone. Certainly motives also, as well as all other states of the organism, involve some feeling, or awareness, of the condition of the organism, and the only difference between the feeling tone of emotion and that of other conditions is that the energy level called emotional represents an extreme departure from the usual energy level and involves, therefore, an unusual heightening or deadening of sensations.[8]

[8] A more comprehensive discussion of the phenomena of 'emotion' may be found in my paper, "An Explanation of 'Emotional' Phenomena without the Use of the Concept 'Emotion'," which is to be published soon in the *Journal of General Psychology*.

The most desirable energy level of response is obviously one which is high enough to enable the individual to reach the goal of his behavior, but no higher than is required for that purpose. Continual recurrence of very high levels of energy is likely to result in the type of physical exhaustion which is called a 'nervous breakdown.' Moreover, a very high energy level may result in disorganized responses. A very low energy level, on the other hand, is usually accompanied by a lack of alertness, as well as by disorganization of behavior. The particular energy level which would represent the best adjustment to the situation will vary with the situation and with the individual making the adjustment.

An inappropriate energy level may be due to any one of a number of causes. It may be due, for example, to glandular dysfunction or to other varieties of physiological maladjustment. On the other hand, it may be due to a faulty interpretation of the situation to which the individual is adjusting, *i.e.*, to an inadequate response to relationships. For example, an individual who sees in every situation a threat to his security, an obstacle to the attainment of his goals, will be continually anxious, wrought up, excited. His energy level will be higher than the situation calls for, but not higher than the situation *as he interprets it* calls for, since he believes that he must be on guard against disaster. The interpretation of a situation made by a depressed individual will be in marked contrast to that made by an anxious individual. The depressed individual will view no situation as significant enough to cause him to arouse himself or exert himself in any way. Because his progress toward an important goal appears to be blocked, other goals will have lost their appeal. He will not interpret adequately their relation to himself; hence he will make no effort to attain them. His energy level will be lower than a satisfactory adjustment to the situation calls for, but it will be appropriate to the situation as he interprets the situation. Abnormal levels of energy, which characterize the behavior of neurotics and psychotics, are frequently the result of the distorted interpretations of situations made by these individuals.

The three concepts which have been described are interrelated, as might be expected from the fact that each represents an *aspect* of a *unitary* response directed toward a particular goal. The energy level of response depends in part upon the interpretation of the situation, or the response to relationships; but it depends also upon the physiological condition of the individual. The particular relationships which will be responded to in a situation are determined by the present direction of the individual's behavior, the goal toward which he is aiming; but they are determined also by his ability to discriminate relationships of the type with which he is dealing, which in turn is determined by his previous experience with similar relationships and by certain factors of individual constitution. The direction of behavior is determined by the nature of the disequilibrium which the individual is experiencing, but the degree to which direction is consistently maintained throughout a response cycle, within the limits of the individual's discrimination of the relationships in the situation, depends upon the energy level at which response occurs and upon the individual's inhibitory abilities.

By making use of the three 'cross-sectional' concepts which I propose, it would be possible to dispense with the loose and overlapping concepts which now constitute our cross-sectional descriptive categories. *Three* concepts, each of which defines a *functional unit* in response, would be sufficient for a complete description of the phenomena now described only by the use of a far greater number of concepts, most of which represent compounds of functions rather than unitary aspects of behavior. For example, the phenomena of set and of attention are merely those of direction in behavior. Selectivity is the functional characteristic of both set and attending. Attention refers to selectivity of implicit responses, while set refers to selectivity of either implicit or overt responses. The phenomena of perception are those of direction in behavior and response to relationships. Perception is selective; hence it manifests direction. It consists of getting the 'meaning' of present sense data, or interpreting these data in relation to each other and to previously experienced situations; hence it

manifests response to relationships. The phenomena of thought also are those of direction in behavior and response to relationships. Thought processes are guided, or directed, by the purpose of the thinking, and they consist of responding to those relationships among symbols which are significant in relation to the goal of the thinking. Perceiving and thinking are thus seen to be similar, rather than fundamentally different, processes. They can be distinguished from each other only with difficulty, and the distinction, when made, serves no purpose in psychological explanation. The phenomena of emotion are those of energy level, direction in behavior, and, perhaps, response to relationships. An individual is said to be emotional when he has an unusually high energy level (excitement) or an unusually low energy level (depression). Or he may be said to be emotional only when his behavior is disorganized (*i.e.*, when the separate responses within the response cycle fail to stay in line with the general *direction* of the cycle). The specific emotions are described in terms of the direction of behavior in relation to the stimulus situation. Response to relationships might also be said to be involved in emotion, since an emotion is aroused by the perception or thought that a certain situation is of great significance (favorable or unfavorable) for the individual. Some psychologists might prefer, however, to say that the emotion *follows*, rather than includes, the perception or the thought. The phenomena of motivation are those of the direction of behavior toward a goal and an increase in the energy level of response. A motive is said to *direct* and to *energize* behavior. The concept of motivation includes also a description of the *origin* of motives, first in tissue needs, and later in other needs of the organism. But, since *all* response originates in a stimulating condition which is a result, or an expression, of disequilibrium of the organism, this characteristic scarcely serves to distinguish a motive from any other psychological process. In practice, we use the term 'motive' to refer to a set (or directional trend in behavior) which persists for a certain length of time because the maladjustment is not readily removed. Thus we seldom use the term motive to refer to needs which

are met by prompt reflex responses; we use it, characteristically, to refer to needs which are less promptly disposed of and which usually involve learned behavior.

Psychologists have too infrequently investigated *as such* those phenomena which appear to be the functional units of response. We have too often observed them merely as incidental aspects of non-unitary categories of behavior such as emotion or thought. A study of the maintenance of direction in behavior, response to relationships, and energy level, and of the conditions producing, and the effects resulting from, variations in these aspects of response would, I believe, provide us with a complete account of the dynamic variables of response and the laws governing their action. By means of such a procedure we should be able to bring together and systematize under a few categories phenomena now treated in many different fields of psychology and divided into many different classifications. The phenomena of genetic psychology, for example, are in large part the developmental aspects of these characteristics of response; the phenomena of differential psychology, the differences among individuals in these characteristics; and the phenomena of abnormal psychology, the extreme variations in these characteristics which, though they are aimed at adjustment, result in socially maladjustive responses.

An an illustration of the systematization of data which might be obtained by the use of the concepts I have suggested, let us consider briefly the systematization which would result from the use of the concept of 'energy level.' By means of this concept we should be able to bring together under one principle most of the data from Cannon's (2) studies of the excited emotions, Jacobson's (18) studies of 'neuromuscular hypertension,' Darrow's (3, 5) studies of the 'excitation background,' Freeman's (12) studies of 'postural tensions' or 'reactivity level,' Luria's (23) studies of the 'psychophysiology of the affective processes,' and the writer's (7, 8, 9) studies of 'muscular tension' or 'degree of excitation'—to mention but a few of the more obviously related studies.[9] We should be

[9] Though the concepts and procedures employed by the various investigators were not identical, it seems probable that the phenomena reported should be related to a

able to formulate certain principles of the relationship between this aspect of response and certain characteristics of the completed response, such as its speed and co-ordination. Freeman (12) has shown, for example, that an increase in muscle tension shortens reaction time, and Duffy (7, 9) has found, in young children, an association between high levels of muscular tension and various indications of inco-ordination of response. We should be able to formulate principles of relationship between stimulus thresholds and energy level. Miller (25) has reported a decrease in the sensation of pain and decreased arm movement from electric shock when the subject was relaxed. Other investigators have reported changes in stimulus thresholds under conditions which appear to involve changes in energy level. We should in all likelihood be able to formulate principles of relationship between the energy level of the organism and the speed of learning. It is generally recognized, for example, that a conditioned response cannot be established unless the subject is alert, and it has been shown by a number of investigators that the response can be established more easily if the general level of muscular tension is increased by the concomitant execution of some voluntary motor response (24). These and many other phenomena could be systematized under the concept of energy level.

The three concepts, (*a*) maintenance of direction, (*b*) response to relationships, and (*c*) energy level, describe the functional characteristics of response, or those qualities of response which determine the adequacy of the behavior as an adjustment; they do not describe the causal factors which produce a given kind of behavior. To describe these factors we need an additional set of concepts, *i.e.*, that set of concepts which I have referred to as the 'longitudinal' concepts of psy-

single basic concept such as that of energy level. A number of investigations have shown that the various physiological measures employed correlate highly with each other. Which particular physiological measure or combination of measures should be regarded as supplying the best empirical definition of the concept of *energy level* remains to be determined. Theoretically, the rate of metabolism might be regarded as most satisfactory, but our present techniques for the measurement of metabolism leave much to be desired. At the present time the measurement of palmar skin resistance, used alone or in combination with some other measure, appears to offer the most satisfactory technique for the study of this aspect of response (4, 5, 13).

chology. The kind of behavior produced at a given moment depends upon the stimuli acting upon the organism at the moment, and upon the kind of organism upon which the stimuli are acting. The kind of organism upon which the stimuli are acting depends in turn upon a group of developmental factors frequently divided into the categories of 'maturation' and 'learning.' An outstanding characteristic of all behavior is its *modifiability*. Modifications, when made, are *retained*, so that the behavior of the future takes its character in large part from the behavior of the past. Behavior modifies the organism, and the modified organism behaves differently the next time it is in a similar situation. The organism thus carries with it its entire life history, and the response made by it at any given moment is due not only to the stimuli acting upon the organism at the moment, but also to the nature of the organism, which has been determined by its entire history of development up to that point. Changes in the three functional qualities of response (direction, response to relationships, and energy level) are thus seen to be due (1) to changes in the stimulus situation, and (2) to changes produced by the previous activities (or developmental history) of the organism.

The present paper represents an initial venture in the modification of that important group of psychological concepts which I have referred to as cross-sectional concepts. If it does not afford an ultimate solution to the problems presented by these concepts, perhaps it constitutes at least a first step toward that revision of psychological categories which must be undertaken before there can be an adequate systematization of the data of psychology.

Summary

Psychologists have in recent years become increasingly dissatisfied with the conceptual categories of our science. These categories, which represent a traditional and in many cases an *a priori* analysis of psychological phenomena, are vague and overlapping. The number of concepts is greater than is necessary to account for the observed variations in psycho-

logical processes, and the concepts fail to describe processes which have *functional unity*.

The attempt to reduce psychological concepts to the smallest possible number of fundamental concepts leads to the proposal that we substitute for our cross-sectional categories which purport to describe the dynamic variables of response (*e.g.*, attention, perception, thought, motive, and emotion) three concepts which, it is maintained, are adequate to account for all variations in psychological responses which are significant in determining the quality of the behavior as an adjustment. These concepts are: (1) the maintenance of direction in behavior, (2) response to relationships, and (3) energy level.

The three aspects of response which these concepts describe are: (1) aspects of *all* responses, overt and implicit, (2) distinguishable but closely related, (3) developed as adjustments to stimulating situations, but (4) independent, in their characteristics, of any classificatory interpretation of the stimulus situation from which they derive. They constitute, it is maintained, the only concepts which are needed for describing the dynamic variables of response, though additional concepts are required for describing the stimulating conditions which give rise to response and, perhaps, certain characteristics of the completed response. Each concept represents, not a combination of several different kinds of psychological processes, but a *functional unit* in response. In this respect the proposed concepts differ from the traditional psychological categories, in which *thought* and *perception*, for example, include the maintenance of *direction* in response and *response to relationships*, *motivation* includes maintenance of *direction* and change in *energy level*, and *emotion* consists of an extreme change in *energy level* or of disorganization of response (*i.e.*, inadequate maintenance of *direction* in behavior).

The concepts here proposed are free from the connotations of 'faculty' psychology; they are already familiar to psychologists in most of their details; and they have in certain instances supplied the conceptual basis of important experimental investigations. It is time now, since we have forged

new and better conceptual tools, to put aside the old ones. By so doing we should give to psychological description a clarity and a simplicity which it now lacks.

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THE EXTERNALIZATION OF DRIVE. I. THEORETICAL CONSIDERATIONS [1]

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A differentiation between internal and external components in motivation has been made by several writers. Katz (10) has stressed the two component theory of drive which suggests that both internal and external factors are important in motivation. The Sewards (13) have suggested that drives may be classified according to their dependence upon internal or external factors. The hunger drive, for example, is primarily dependent upon internal factors within the organism for its arousal, whereas the exploratory drive is aroused by the presence of a particular type of external situation rather than by changes taking place within the organism. The writer (2) also found it convenient to classify drives according to their dependence for arousal upon internal and external factors in order to account for the results of a correlational study on motivation in rats.

In addition to the recognition of the two factors in the arousal of motivated behavior, the internal and external, there has also been an increasing recognition of a shift in dependence from one set of factors to another. This shift has been emphasized by Allport (1) in his theory of the functional autonomy of motives which implies that drives lose their dependence upon the original conditions and come to function as independent motivational systems. In accounting for some of the results of the motivational study mentioned above, the writer also found it desirable to emphasize a shift from one set of factors to another in the *arousal* of the drive.

For the purpose of this paper it will be assumed that some drives, in their earliest manifestation, are dependent for their

[1] This paper is the first of a series. The other parts are experimental and will appear elsewhere.

arousal upon conditions within the organism while others are dependent for their arousal primarily upon the external situation. Some overlapping may be expected but, in general, the differentiation is relatively clear. The hunger drive, for example, appears to be aroused by changes taking place within the organism such as stomach contractions, changes in blood composition, etc. This by no means equates the hunger drive with these physiological conditions. The basic assumption is that there is some neural or neuro-muscular or neuro-glandular mechanism which leads to persistent behavior. This neural mechanism is aroused by or stimulated or excited by the physiological changes taking place within the organism due to the deprivation of food. The term *need* might be used to refer to the internal physiological state which arouses the neural mechanism (the *drive*) that results in persistent behavior. The exploratory drive, on the other hand—again considering the neural mechanism which produces the persistent behavior we call exploration—is aroused not by physiological changes within the organism produced by deprivation but rather by a certain type of external situation, namely, a novel stimulus constellation. At present, it does not appear possible to state whether the neural drive mechanism aroused by different internal or external conditions differs with the conditions that arouse it or remains the same in spite of differences in the conditions of arousal. Conceivably, physiological conditions as different as those of hunger and thirst might arouse the same neural mechanism leading to persistent behavior. There would, of course, also be a difference in the end product (food or water) which would allay the physiological condition, but the neural mechanism might be essentially the same. On the other hand, it is also conceivable that the neural factors might differ with the different physiological conditions. Ultimately, the solution of this problem should be of considerable theoretical importance but does not appear to be essential for the purpose of this paper.

If changes in the conditions which lead to the arousal of a drive occur, they may take several different directions. If we assume a drive X originally aroused by physiological changes

within the organism, that is to say, internally aroused, then the following changes in conditions of arousal might take place: (1) Originally aroused by a set of internal conditions I-1, the drive might become aroused by a new set of internal conditions I-2. For example, if the internal conditions I-1 which normally arouse the hunger drive, drive X, were frequently associated with a rise in blood pressure (internal conditions I-2) then it might sometimes happen that the new conditions, I-2 might in time arouse drive X in the absence of the hunger state.[2] (2) If a certain external situation E-1 is frequently present in conjunction with the internal conditions I-1 which originally arouse drive X, then, in time, the external situation E-1 may itself arouse drive X without the presence of I-1. For example, Bayer (see **10**) has found that after a chicken has eaten as much grain as it wishes it may be induced to eat more merely by presenting it with more food of the same kind. The external situation thus arouses the drive behavior in the absence of the internal conditions related to hunger. (3) Through a process similar to higher order conditioning, it is obvious that further changes in the arousal of a drive may take place. Other external situations, E-2, E-3, E-4, etc., may, by being associated with I-1, I-2, E-1, etc., come to arouse the drive X. (4) The drive having become dependent upon a given external situation, say E-1, further association may lead to dependence upon an entirely new set of internal conditions, I–X, and the drive may be said to be re-internalized.

In the case of a drive originally aroused by an external situation, constant association of that situation with other external stimuli or with internal conditions of the organism may result in a change in the conditions of arousal. In some cases, the result of a given activity may actually produce physiological conditions in the organism which create an entirely

[2] It is obvious that this process is closely related to conditioning. In conditioning, however, reference is usually made to a particular type of behavior or response. In the present case no assumption is made concerning the identity or similarity of behavior. It is the persistent behavior or drive which is being conditioned. If the term conditioning is used, therefore, we should speak of a conditioned *drive* rather than of a conditioned response.

new need. This latter case is illustrated by the establishment of a drug habit. Originally an individual may be persuaded by his companions to take a drug; the social situation, therefore, appears to stimulate the individual to a certain activity. After a few indulgences, however, the drug causes internal changes which arouse the drive for that drug and the drive is then no longer dependent upon the original social situation which first aroused it. The originally externally aroused drive would now have become internalized, or internally aroused.

The present paper will be concerned with only one of the above types of change, namely, the shift from original dependence upon internal conditions to dependence upon external conditions and the discussion will be primarily about the hunger drive in the rat and its relation to maze performance. For the present purposes, it will be assumed that the hunger drive is originally aroused by physiological conditions within the organism, that is to say, it is an internally aroused drive. If this internally aroused drive is satisfied over a long period of time in a relatively constant external situation, then the drive mechanism will become aroused by this external situation. We may now say that the originally internally aroused drive has been externalized. It is also assumed that the process of externalization gradually spreads from one external stimulus constellation to another so that, in time, practically any stimulus object that has been associated in any way with the drive may come to arouse that drive and should lead to the type of persistent behavior characteristic of motivated actions. No assumption is made concerning the actual identity of the stimulus conditions necessary to arouse a drive, but it is assumed that externalization spreads to similar stimulus constellations which are roughly equivalent for the organism. The shifts in motivation described above may be described in terms other than those used here (Hull's conditionings, Tolman's hypotheses, Lewin's valences, etc.) but certain implications seem to follow most readily in terms of the present terminology.

In the case of an organism such as the rat learning a maze under normal conditions of hunger motivation and food re-

ward, it would seem apparent that, in the early stages of learning, the drive involved is primarily aroused by the internal conditions of the organism.[3] The animal is learning the maze and is using that knowledge to run the maze correctly because he is hungry. As the maze training continues, however, and the animal is given an increasing number of trials under the normal conditions of hunger and reward, the drive gradually becomes externalized and can be at least partially aroused by various characteristics of the external situation. If complete externalization could be attained, it would presumably be possible to arouse the drive to almost full strength by merely presenting the appropriate external situation even in the absence of the internal conditions that originally aroused the drive.[4] It does not seem probable that such complete externalization takes place in the rat, but it may be expected that some degree of externalization will occur. Externalization probably occurs first to the food reward (in the case of the hunger drive) although it is possible that it may occur to several aspects of the external situation simultaneously. The process then probably spreads to the food box and finally to the general characteristics of the maze itself (the alley, curtains, retracing doors, choice point characteristics, etc.). After many trials, therefore, general characteristics of the external situation should arouse the drive mechanisms to some degree. The internal conditions which aroused the drive, of course, still continue to do so, but the external factors have become relatively more important than they were earlier in learning. In the late stages of learning, with both the internal and external factors operating to arouse the drive, the degree of arousal would presumably be maximal.

[3] A certain amount of externalization of drive might take place during the preliminary orientation training which usually precedes maze learning, or even in the living cages before any experimental procedures have been introduced. The statements made must always be taken in a relative sense, *i.e.*, the external factors are *less* important early in maze training than they are later.

[4] The internally aroused drive results in characteristic persistent behavior even in the absence of the external objects that might allay the physiological condition. The hungry animal, for example, becomes active even though no food is present in the environment. With the externalized drive, on the other hand, the external situation arouses the characteristic drive behavior even in the absence of the internal conditions originally important.

Essentially, the theory outlined above implies that the relative importance of the internal and external factors varies at different stages of learning and that the results to be expected from altering the internal or external factors will depend upon the relative importance of those factors. A number of specific implications follow from the theory.

1. In the early stages of learning when the internal factors are more important than the external, satiation, which disturbs the internal conditions of the organism, should produce a greater increase in errors[5] than would removal of the reward, the latter merely changing the external situation. In the later stages of learning, however, a greater degree of externalization can be expected to have taken place (internal conditions may still play a role but they should be less important than they were in the earlier stages) and alteration of the internal conditions late in learning should produce less disturbance than such alteration would produce before externalization has occurred. Therefore:

2. Satiation at a late stage in the learning process should produce relatively less impairment in error score than it produces earlier in the learning process. After externalization has occurred, the external situation is of greater importance than it was earlier and alteration of the external situation should result in a greater disturbance of performance than such alteration would produce early in learning. Therefore:

3. Late in learning, removal of reward should result in a greater increase in errors than would removing the reward earlier in the learning process, *i.e.*, before externalization has taken place. There is a complicating factor here since the removal of reward takes away only one aspect of the external situation. If externalization had spread to the food box itself, removal of reward late in the learning process might produce a relatively minor disturbance. Under ideal conditions, if the

[5] Most of the implications considered are stated in terms of error scores. It is probable that many of the propositions are also true for time scores, but time scores appear to be somewhat unsuitable measures in some cases because they are easily affected by extraneous variables which may introduce complicating factors. This is especially true in comparing the performance of satiated with hungry non-rewarded animals.

experimental conditions could be introduced at a time when the drive was primarily aroused by the food in the food box but not by the food box nor the general maze situation, then removal of reward might be expected to produce a very marked impairment. On the other hand satiation, which would change the internal conditions only, might be expected to produce a relatively minor disturbance. Under some conditions, then:

4. Satiation late in learning may actually produce less impairment in error score than removal of reward. Several complicating factors, however, make this proposition subject to error: (*a*) Late in learning, externalization probably involves the entire stimulus complex. Removal of reward does not take away the entire external situation to which the drive has become externalized, but only one part of it. (*b*) The internal physiological state of hunger is known to facilitate general activity (12) and Elliott and Treat (8) have shown that the hunger contractions of the stomach facilitate learning in a situation in which hunger was not the primary motivation leading to learning. (*c*) If animals have been upon a restricted diet for some time, as is the case with rats learning a maze, satiation might be expected to produce a marked physiological upset in the animals (a sort of 'satiation shock' effect). This shock effect would presumably be greater the longer the animals had been on the restricted diet, and might introduce variables over and above the satiation state itself. (*d*) The effect of satiation would appear to be immediate, whereas, after removal of reward, the animal may require a few trials to learn that the food reward is no longer present. All of these factors would appear to favor the removal of reward condition and to handicap the satiated animals. Instead of predicting that, late in learning, satiation will result in less disturbance than removal of reward, it would appear sounder to say that:

4a. Early in learning, satiation will produce a greater disturbance in error score than will removal of reward; later in learning, the disturbance produced by satiation will be more nearly equal to that produced by removal of reward and might,

under some conditions, be less. If complete externalization could be obtained in the rat, removal of the internal conditions that arouse the drive mechanism by satiation should leave the performance unimpaired. It seems unlikely that such a complete stage of externalization will be obtained in the rat; thus satiation will probably produce some disturbance in performance, as indicated by an increase in error score, regardless of the stage of learning at which it is introduced. However, after externalization has occurred, changes in the external situation (which serves to arouse the externalized drive) combined with satiation should produce a greater disturbance in performance than satiation alone. Therefore:

5. Late in learning, satiation plus removal of reward (even though the animals may not eat a reward) should result in a greater increase in errors than satiation alone (*i.e.*, with food present in the reward box). Whereas:

6. Early in learning, before externalization has taken place, satiation plus removal of reward should be no more detrimental, or only slightly so, than satiation alone. Since the substitution of a new reward box at the end of the maze for the one to which the animals have become accustomed is also a change in the external situation, and if the training has been continued long enough to produce externalization to the food box, then:

7. Late in learning, satiation plus a change in the reward box should result in a greater increase in errors than satiation alone. Whereas:

8. Early in learning, satiation plus a change in the reward box should cause little or no more disturbance in the learning than would satiation alone. In the case of hungry rewarded animals, late in learning, both internal and external factors are operating and the drive is aroused maximally so that a change in the external situation should produce an impairment of performance. Hence:

9. After externalization has occurred, a change in the reward box of hungry rewarded animals should produce some disturbance in performance. The disturbance can be expected to be quite slight and probably very temporary since

an important aspect of the external situation (the food) is still present, and since it is likely that experienced hungry rewarded animals will show very rapid externalization to a new food box. Using a simple maze yielding time and excess distance scores, Bruce (6) was able to demonstrate that a change in the reward box (without removing the reward) results in a temporary disturbance of performance. But:

10. Early in learning, before externalization has occurred, change in the reward box for hungry, rewarded animals should not result in disturbance in performance. After externalization has taken place, removal or change of more than one aspect of the external situation may be expected to result in a greater increase in errors than would removal or change of but one part of the external situation, while early in learning such an added alteration of the external situation should have little or no effect since externalization has not yet taken place. Hence:

11. Late in learning, removal of reward from hungry animals combined with a change in the reward box should result in a greater increase in errors than removal of reward alone or change of reward box alone. But:

12. Early in learning, removal of reward combined with change in reward box should produce no greater increase in errors than removal of reward alone. The same effects may be expected with satiated animals, that is to say:

13. Early in learning, satiation combined with change in reward box and removal of reward should be but little or no more detrimental to performance than satiation plus change in reward box or satiation plus removal of reward, or satiation alone. But:

14. Late in learning, satiation combined with change in the reward box and removal of reward should result in a greater increase in errors than satiation combined with either of the changes singly or satiation alone. In this case the internal conditions which might arouse the drive are removed by satiation and two aspects of the external situation which might arouse the externalized drive are altered or removed. The resultant increase in errors should therefore be greater

than for any of the other situations described. It has been suggested that externalization probably occurs first to the food reward and it seems likely that, at some stage of development at least, this externalization will be to the particular kind of food which has been used in the reward situation. A change in the kind of food found in the food box thus constitutes a change in the external situation involved in the arousal of the externalized drive. Thus:

15. Early in learning, before externalization has occurred, a change in the kind of food reward should have little effect upon performance. The experimental testing of this point is complicated by the fact that externalization to a particular kind of food may have occurred in the animals' living cages or during preliminary orientation training. Such factors must necessarily be adequately controlled. On the other hand:

16. Later in learning, after externalization to the particular food has occurred, a change of the food reward should result in a disturbance of performance. This, of course, is the familiar 'anticipation' effect demonstrated by Elliott (7). Externalization to a particular kind of food may occur in the animal's living cages during the preliminary training period, or even during the daily feeding period after maze running has begun. If such externalization to a given kind of food has been established outside of the maze situation, the introduction of that food into the maze situation should intensify the arousal of drive and hence improve performance. Therefore:

17. If rats are fed food A for a period of time and if their daily rations during the training period on a maze consist of food A, but if the reward found at the end of the maze is not food A but is food B, then the introduction, after a few trials, of food A as a reward in the maze (instead of B) should *improve* performance. This would be the opposite of the 'anticipation' effect. The experimental test of this proposition requires several precautions: (*a*) The training with food B should not be so long that externalization to food B occurs in the maze situation. Such externalization might be avoided by using a different reward in the maze each day during the food B period. (*b*) The rats should be able to discriminate

between the foods, but, preferably (*c*) should show no marked preference for one food over the other. (To a certain extent, of course, externalization to a given food would result in a preference for that food.) A somewhat similar situation would arise when externalization to a given food has been established outside the maze situation and this food is then introduced to previously non-rewarded animals. That is:

18. If the customary food and the daily rations during the training period on a maze is food A but if no food reward is actually given in the maze situation for a number of tirals, then the introduction of food A as a reward in the maze should improve performance. This is the familiar 'latent learning' effect adequately demonstrated by the California investigators (**4, 18**). It is, of course, probable that the introduction of *any* reward in a similar situation would improve performance, but the theory of externalization would imply that this improvement should be more marked when externalization to the food used as a reward had been established previously. That is:

19. If rats are fed food A for a period of time and if their daily rations during the training period on a maze consist of food A, but if there is no reward in the maze for a number of trials, then the introduction of food B (entirely new to the rats) as a reward in the maze should result in a less immediate and less marked improvement in performance than would be obtained in the situation described in the preceding proposition 18. The precautions suggested under proposition 17 should also be considered here. The preceding propositions suggest that the reward does more than merely 'satisfy' the animal. Once externalization has occurred, the presentation of an appropriate aspect of the external situation is sufficient to arouse the drive mechanism which leads to persistent behavior and the reward is one such aspect of the external situation. Therefore:

20. Presentation of a reward immediately before the animal is given his trial on the maze should improve performance.

21. This improved performance should be obtained even if no reward is present in the reward box at the end of the maze. But:

22. Early in learning, presentation of a reward before the animal runs the maze should not produce improvement in performance. It might actually impair the performance since the pre-reward would tend to allay the physiological state important in the arousal of the drive at this stage. Suitable controls must be used to prevent externalization to the food before the maze experiment is begun. Bruce (5) has demonstrated the truth of proposition 20. After externalization has occurred, the reward has a complex function; it not only serves as a 'reward' that satisfies or allays the internal physiological condition, but also plays a part in the arousal of the externalized drive. Bruce (5) found that the effect of the pre-reward procedure was greater for the thirst drive than for hunger. It is possible that the two drives differ with respect to this point, but there is an alternative explanation of this finding. It would seem probable that externalization takes place most rapidly when the external situation remains relatively constant. Thus, externalization to the reward occurs most rapidly when the reward remains the same from trial to trial and would be slower if different rewards were used from time to time. In Bruce's experiment, the hunger group received different foods as a reward during the first twenty trials. Under these conditions, externalization to a given food would be expected to be slow. Presentation of the pre-reward would therefore have a relatively slight effect because adequate externalization to the particular food involved had not been attained (proposition 22). With the thirst group, on the other hand, the same reward (water) was given each trial and externalization would be at a normal rate. It would also seem probable that some degree of externalization is necessary before complete learning occurs. Hence, other things being equal, learning will be the more rapid as externalization is more rapid. Thus, animals learning a maze with a different reward each trial should be inferior to animals learning with a constant reward. This result was obtained by Bruce (5). Since the presentation of the reward object itself appears adequate to arouse the externalized drive:

23. Under some conditions the presentation of food to satiated animals should be sufficient to result in the eating of that food. Bayer (reviewed by Katz, 10) has studied a number of conditions under which such behavior is obtained in the chicken. The truth of the present implication is also indicated by the common laboratory observation that the mere addition of more food to the container in rats' living cages is often sufficient to induce a number of animals to begin eating. The present point presents some difficulty. The fact that after externalization has occurred some 'satiated' animals eat the reward found at the end of the maze may cause some persons to doubt whether the animals were satiated.

The fact that some drives are originally aroused by external situations has further implications for the theory of externalization of drive. Any relatively new situation may be expected to arouse, in the rat, the exploratory drive, emotional reactions such as timidity, and possibly other types of reactions. Before a given internally aroused drive can become externalized to a given situation, it would appear to be necessary that the situation should cease arousing exploratory or timidity reactions, *i.e.*, that negative adaptation for these types of behavior should take place. If an animal is very timid, or has a very strong exploratory drive, some effect upon the process of externalization may be expected. Therefore:

24. In very timid animals, externalization should be slow in developing or may not occur at all. The experimental test of this point offers some difficulties. A measure of timidity may be obtained by using one of the procedures described by Hall (9) or by the writer (3), but a measurement of the speed of externalization may be hard to get. Some indication could be obtained by introducing satiation periods at various stages of learning. Rats showing relatively little impairment in performance during the satiation period would be considered externalized. The speed of externalization could then be measured in terms of the number of trials necessary before satiation produces a relatively small impairment. Another approach would be to correlate the measures of timidity with the increase in errors produced by satiation after a specified

number of trials on a maze under normal conditions of hunger and reward. With both of these approaches the problem as to whether to use relative or absolute increase scores may prove troublesome. Some modification in the methods of scoring timidity may also be necessary since present measures do not differentiate adequately in a single score between animals showing a timidity reaction persisting over a long period of time (which is the type of timidity that would presumably interfere with externalization) and animals showing a very high timidity score for a few days and then rapid adaptation. At the human level, some differentiation between introverts and extroverts might be made on the basis of the degree of externalization attained. If introversion involves timidity, the external world (especially in its social aspects) might be expected to arouse a fear reaction in the introvert and externalization would therefore be relatively difficult. The extrovert, being less timid, might be expected to show externalization more readily. For the extrovert, then, the external world would serve to arouse one or more externalized drives and thus lead to activity, while the introvert would be more dependent upon internal sources of motivation since the external situation is likely to arouse an emotional reaction. For reasons similar to those considered above:

25. In animals with a very strong exploratory drive, externalization should develop slowly or not at all. The experimental test of this point will involve the same difficulties described for proposition 24. Measurement of the speed of externalization would be as suggested previously. Measurement of the exploratory drive should involve no special difficulties (see **2**, *e.g.*) but it may be necessary to differentiate between animals showing a relatively strong exploratory drive over a long period of time and those showing a very high exploratory activity for a time and then relatively rapid adaptation. In an earlier paper (**2**) the writer presented reasons and evidence for the suggestion that different measures of externally aroused drives yield higher intercorrelations than similar measures of internally aroused drives. Intercorrelations between different measures of a drive such as

hunger should therefore increase as externalization proceeds. Hence:

26. With a drive such as hunger, long continued testing upon different kinds of apparatus used in measuring drives should yield higher intercorrelations among the different tests than would be obtained without such practice. And, as a corollary to this:

26a. The reliability of tests involving drives such as hunger should increase as externalization occurs. Applied to the maze problem, the reliability coefficients for the early trials on a maze should be less than for later trials (provided that the later trials yield sufficient differentiation of scores). There is evidence that such is the case. Studies of the reliability of mazes and other problems (**11, 14, 15, 16**) indicate that the odd-even reliability coefficients for trials 11–20 are usually higher than those for trials 1–10.

If training upon one maze under constant conditions of hunger and reward is continued for a very long time, it may be expected that externalization will become maximal and will spread not only to the food box but also to the general characteristics of a maze. Presentation of an entirely different maze should then arouse the externalized drive and thus lead to a motivated performance. Therefore:

27. Rats that have had prolonged training on one maze under normal hunger-reward conditions to produce externalization may be expected to learn a new and different maze in the absence of the normal hunger and reward conditions. The externalized drive is here the primary source of motivation for the learning of the second maze. More specifically, after such externalization training in one maze, rats can be expected to learn another maze under conditions of hunger—no reward, satiation—no reward, and satiation—reward. Since individual differences are known to exist in practically all functions, it is to be expected that they will be found with regard to the degree of externalization attainable (it has already been suggested that individual differences in externalization may be related to differences in traits such as timidity and the exploratory drive). Thus not all rats can be expected to

learn the second maze under the conditions suggested. Those animals in which externalization is most complete may be expected to learn the second maze under the anomalous conditions of motivation and a few animals might learn it as rapidly as would animals running under normal conditions of hunger and reward. The externalized groups as a whole should be superior to control groups trained on the second maze under comparable conditions of motivation but without the previous externalization training. Groups run under the various motivational conditions suggested above may be expected to show some differences in performance upon the second maze. That is to say:

28. In the situation described in proposition 27, satiated rewarded animals should be superior to satiated non-rewarded animals. This would follow because one more aspect of the external situation (the food) is present for the rewarded animals. It may be, however, that once the high degree of externalization necessary has been obtained, the externalization may be to the total situation and would not be markedly affected by changes in the details of the situation.

29. In the situation described in proposition 27, hungry non-rewarded animals should be superior to satiated non-rewarded animals. In both cases, the external situation is comparable but the hungry non-rewarded animals have the advantage of the facilitating effect of the hunger conditions previously mentioned and should therefore be superior. As a corollary to this:

29a. In the situation described in proposition 27 and with animals learning the second maze under conditions of satiation no-reward, making the animals hungry (without, however, introducing a reward) should result in an improvement in performance. A statement comparing the performance of hungry non-rewarded animals with satiated rewarded animals on a second maze (after externalization on a first maze) offers somewhat greater difficulties. The satiated rewarded animals have the advantage of an additional aspect of the original external situation (*i.e.*, the food) which may arouse the externalized drive. On the other hand, the hungry non-rewarded ani-

mals have the advantage of the facilitating effect of the hunger condition. It does not appear possible to state in advance of experimental determination which of the two conditions should have the greater facilitating effect upon performance. The experimental determination should not be difficult: a comparison of hungry non-rewarded animals with satiated non-rewarded animals would indicate the facilitating effect of the hunger condition, while a comparison of satiated rewarded animals with satiated non-rewarded animals would indicate the extent to which the food reward operates in arousing the externalized drive.

Phrases such as 'early in learning,' 'late in learning,' 'before externalization has occurred,' 'after externalization has taken place,' etc., have been used in the preceding discussion without precise definition. Several possibilities of definition may be suggested. (1) Since externalization proceeds as experience in a relatively constant situation continues, it may be assumed that more externalization has occurred after 15 trials in the maze than after 7 or 8, still more after 30 trials and yet more after 50, etc. 'Early in learning,' 'late in learning,' etc., may therefore be given a preliminary definition in terms of an arbitrary number of trials in the maze, the actual number depending upon the amount of preliminary training given, the difficulty of the maze, etc. Such a definition would be adequate for group comparisons in studies involving the determination of performance at different stages of externalization, but gives no information concerning the individual animals. Nor does it give a precise indication of the point at which externalization first begins to influence performance. For purposes of preliminary experiments, however, it would seem satisfactory to assume that little externalization has occurred within the first 7 or 8 trials on a maze such as the standard 14 unit T-maze. (2) For more adequate determination of externalization in an individual animal some procedures such as those suggested under proposition 24 might be used. These are apt to be very tedious, however. (3) Some indication of the point at which externalization begins to take place may be obtained in the following way. Hungry non-rewarded ani-

mals show a reduction in error score for a few trials on a maze and then the error curve levels to a plateau (17). The lowest error score attained by such a hungry non-rewarded group might be taken to indicate the amount of learning that can occur in the absence of externalization. Therefore, the trial at which hungry rewarded animals drop below the plateau level of the hungry non-rewarded animals may be taken to indicate the point at which external factors are assuming importance. In a curve presented by Tolman and Honzig (17, Fig. 2) the hungry non-rewarded group reached a plateau level at about the twelfth trial. The hungry rewarded group dropped below this plateau level at about the seventh trial. It may therefore be assumed that little or no externalization had occurred until about trial 7. This point, of course, is the beginning of externalization and the actual degree of externalization would not be expected to be high. (4) A modification of the preceding method might also be used. The curve of Tolman and Honzig (17) shows that the hungry non-rewarded group and the hungry rewarded group made about the same number of errors for a few days, the rewarded animals then began to make fewer errors than the non-rewarded rats. The trial at which the error curves for the two groups separate might be taken as indicating the point at which external factors are assuming importance. This is at the fifth trial in the curve of Tolman and Honzig. (5) A further method which may prove to be useful for individual cases as well as for groups involves the determination of the types of errors made by the rats in the early trials of the maze. For present purposes, it is necessary to differentiate between factors which operate to produce errors in certain blind alleys from the very first trial of the maze (centrifugal swing, *e.g.*) and those factors which do not operate until after a few trials on the maze (anticipation, *e.g.*). Anticipatory errors occur when the animal anticipates the next correct turn in the maze or the final correct turn, etc. It would seem probable that some degree of externalization is necessary before anticipatory errors will appear; the trial at which the anticipatory errors become dominant, then, would offer a means of determining when externalization occurs.

Unfortunately many maze patterns are so arranged that factors such as centrifugal swing, which operate from the first trial, will cause errors in exactly the same blind alleys that will later be influenced by anticipatory factors. For purposes of determining externalization it would be necessary to have a maze so constructed that factors operating from the first trial would cause entrances into one set of blind alleys, while anticipatory factors would cause entrances into an entirely different set of blind alleys. The double alternation pattern constructed of U-units appears to fulfill these requirements.

It is possible that, after externalization has occurred, a further stage of development in the arousal of drive may occur in some organisms. That is to say, having become arousable by certain external situations, the drive may now be aroused by an entirely new set of internal conditions. This new set of internal conditions might be cortical in nature and would presumably be very different from the internal conditions which originally aroused the drive. 'Re-internalization' could be used to describe this stage of development. Once a drive has been re-internalized, the new internal conditions within the organism would be sufficient to arouse persistent behavior in the animal; instead of waiting for some external situation to arouse an externalized drive or of being excited only by internal conditions produced by deprivation, the organism would tend to show active seeking behavior even in the absence of clearly identifiable internal or external conditions. To use the rat as an example:—If re-internalization could be attained the animal might be expected to go about looking for new mazes to learn rather than merely learning those mazes which are put before him. It seems unlikely that such a stage of development can be attained in an organism as low as the rat, but no definite statement can be made since experimental procedures which might determine the point are not yet available.

The externalized drive appears to be closely related to Allport's autonomous motive (1). It seems probable that several distinct types of autonomous motives may be included under the general heading of functional autonomy and the

externalized drive could be one such type or class. Presumably the general principles of functional autonomy would apply to all classes of autonomous motives including the externalized drive. But each class may also have its own special principles which would be restricted to that class alone. The special principles may be useful for predicting behavior in concrete situations such as those dealt with in this paper.

To summarize, it is assumed that a drive such as hunger is originally dependent for its arousal upon internal conditions of the organism but that, through continuous use of the drive in a relatively constant situation, the drive becomes aroused by an external situation in the absence of the original internal conditions, *i.e.*, becomes externalized. On the basis of this theory, some 29 predictions were made. Confirmatory experimental evidence is available for a few of the propositions but the majority of the implications have not yet been subjected to experimental test.

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DISCRIMINATION LEARNING AND PRE-DELAY REINFORCEMENT IN 'DELAYED RESPONSE'[1]

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In hunger-motivated maze learning and discrimination learning it has been demonstrated that within limits learning scores of animals depend upon degree of hunger or amount of reward, other factors being held nearly constant. Likewise it seems an axiom that strength of food-hunger conditioning of both the Pavlovian and Thorndikian varieties is decidedly related to amount of hunger or food reward. These important relationships have been studied in some detail, but they have proved to be neither simple nor yet well understood. In regard to 'delayed response,' a neglected but useful technique for testing short term retention, there is evidence that similar relations obtain for the reward factor, at least with degrees of 'reward-expectancy' during the pre-delay (presentation) trial (4), as well as with varying amounts of food reward upon the post-delay (test) trial (10). To date, however, there have been reported no systematic tests of the effect of quantitatively varying actual food reinforcement upon the pre-delay trial. This blind spot in psychologists' scrutiny of important variables in various learning situations may be largely due to prevalent arbitrary delimitation of 'delayed response'[2] to be discussed forthwith.

On the basis of the common evidence on reward in learning, the writer in a recent report of experiments with delayed response predicted that results with his method involving two successive runs to a given goal box, the first *without* food re-

[1] The writer is grateful to Dr. John L. Finan of Oberlin College for many helpful discussions on the topic of 'delayed response.'

[2] In view of the necessarily frequent use in this paper of this rather vague and misleading term, the quotation marks are hereafter omitted except for added emphasis upon its status as merely a familiar label.

ward and the second (test run) *with* food reward, might be bettered by actually giving the animal food on the first, or training, run (2). This latter procedure was expected to yield results more comparable to those obtained by Maier (9), who demonstrated remarkably long retention for the white rat. His experiment allowed the animal two, rather than one, pre-delay runs to food. The use of food reward on the pre-delay runs in these experiments meanwhile arouses the whole question of what constitutes the behavior, or experimental method, referred to as delayed response. In some quarters, stimulated probably by Maier's phenomenal results, serious doubt has been expressed that such a procedure is genuine delayed response, since the animal upon the presentation trial receives not a mere signified, or derived reward but the actual food reward itself.[3] This, it has been argued, is simply position discrimination training. The writer has taken exception to this arbitrary inference, since as yet there seems to be no clear or convenient psychological distinction between the learning-and-retention with signified, or derived reward and learning-and-retention with a certain amount of food;[4] neither can we usefully differentiate on this basis between the experimental procedures for delayed response and discrimination learning. A mere quantitative continuum of pre-delay reinforcement hardly guarantees any discontinuous qualitative differences in resultant behavior (2).

Another supposed distinction between delayed response and discrimination learning, as exemplified by orthodox ex-

[3] For references see (2, page 104, footnote 2). Crannell (5) has recently performed an experiment with rats with a set-up employing four alternative pathways, which were to be run successively in whatever order the rat selected, no second run of any one path being permitted. Delayed response tests were instituted by interposing a delay between the third and fourth runs, a correct post-delay response being the choice of the (fourth and remaining) pathway not previously chosen. Each run was rewarded with food. Successful delays up to 4 hours were observed, even though the animals did not maintain any stereotyped sequence of pre-delay runs in repeated tests. This progressive elimination of three paths constitutes the series of training trials which determines the rat's response after delay. The criticisms of Maier's experiment, by those denying it delayed response status, would undoubtedly be applied to this one.

[4] This no-food situation is not to be confused with the 'latent learning' maze situation, in which the animal has never before associated the correct maze path with food reward. There is no 'latent learning' here, in the accepted sense.

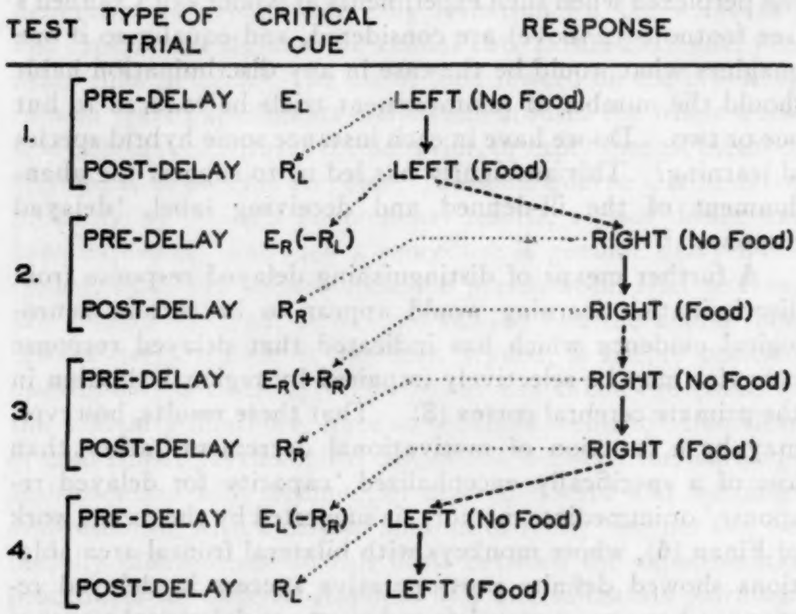
periments, seems to be the obvious dependence of retention in the former upon a *single* reinforcement, or pre-delay trial, while in the latter it depends upon *numerous*, cumulative reinforcements in similar trials. This distinction, however, leaves one perplexed when such experiments as Maier's or Crannell's (see footnote 3, above) are considered, and equally so if one imagines what would be the case in any discrimination habit should the number of reinforcement trials be reduced to but one or two. Do we have in each instance some hybrid species of learning? This ambiguity has led us to suggest the abandonment of the ill-defined and deceiving label, 'delayed response.' [5]

A further means of distinguishing delayed response from discrimination learning would appear to be certain neurological evidence which has indicated that delayed response behavior may be selectively impaired by regional ablation in the primate cerebral cortex (**8**). That these results, however, may be a function of motivational decrement rather than loss of a specifically encephalized 'capacity for delayed response,' or immediate memory, is suggested by the recent work of Finan (**6**), whose monkeys with bilateral frontal area ablations showed definite post-operative success in delayed response when a more complete and overt pre-delay goal response was allowed. Does delayed response become a unique or higher order of behavior when it is made more difficult for the animal by less favorable techniques?

What does indeed promise to be a valid distinction between the learning situations in question is that delayed response requires the concurrent acquisition and performance of two or more alternative habits, while discrimination learning ordinarily centers upon the acquisition of a single habit to the exclusion of others. In other words, delayed response demands immediate but temporary fixation (or rather relearning) of a pre-delay association, followed by a post-delay test, with subsequent irregular shifts to an alternative pre-delay association

[5] This term is deceiving, for it implies that the pre-delay trial is merely a stimulus-situation, the response to which is held in restraint until opportunity is furnished at the post-delay trial. It also obscures the critical responses during the pre-delay trial, which are of prodigious significance.

and test; whereas, discrimination learning demands only gradual but relatively permanent acquisition of the one stimulus-response association. Delayed response may therefore be regarded as a discrimination problem involving the irregular



Schema of Four Successive Tests in Spatial Delayed Response

This illustrates the common paired-trial procedure with a single pre-delay training trial without food reward, and with only two alternative goals. All responses here are without error.

E = exteroceptive cue; R = right, L = left.

R = retained effect (internal cue) of previous response.

. . . . . = derivation of retained effects serving as facilitory (+) or inhibitory (−) cues.

——— = adaptive transfer } Remote transfer is disregarded here.

– – – – = non-adaptive transfer } Remote transfer is disregarded here.

Note: The effect of *inter*-test retention must be greatly attenuated in the successful animal.

'reversal' of the antagonistic habits concerned.[6] Moreover, for continued successful delayed response the animal must not allow the required positive transfer from pre-delay situation to post-delay (test) situation to influence significantly the next following pair of trials. See figure. To reduce this possible

[6] A procedure for obtaining delayed response performance by first employing frequent reversal of individually learned discrimination habits has been reported by Nissen, Riesen, and Nowlis (11).

perseveration from one pair of trials to the next, Adams (1) and others have adopted a rule that the time interval between two such pairs be at least three times the delay interval within any pair. Although the interval between successive tests may be an important factor in delayed response performance, such a three-to-one rule cannot be applied indiscriminately. It has been demonstrated repeatedly to the writer in certain of his experiments that the delay interval may equal or far exceed the interval between tests and yet successful performance is not noticeably impaired (2, pp. 112ff.; 4, pp. 350, 356). The optimal inter-test interval must in any case be empirically determined, but it is very likely more dependent on other features of the experimental situation than the delay interval employed.[7]

If there is any significant distinction, then, between delayed response and ordinary discrimination learning, it may be stated as follows: In delayed response the animal's choice on any *training trial* is determined by explicit differential cues encountered during that trial, while his choice upon any *test trial* is determined primarily, if not solely, by his response upon the *one or more* training trials immediately preceding the test trial; in discrimination learning, however, the animal's choice is primarily determined upon *any trial* (except the first trial) by his responses on *any or all* of the preceding training trials, and, unlike the *several* alternatives in delayed response, only *one* stimulus-response association is favored for fixation. In either case, the particular training procedure and sequence of trials effects the successful animal's adoption of the appropriate problem-set: *i.e.*, periodically interrupted or continuous transfer. Hence, ordinary discrimination learning is the limiting case of delayed response in which *every* preceding trial is a training trial, consistently reinforcing a *single* association.[8]

[7] For instance: species of animal, manner and ease of discriminating the alternative goals, previous delayed response experience, emotional factors.

[8] This paragraph is meaningful only if *spatial* delayed response is compared with *spatial* discrimination; or, similarly, if the corresponding *non-spatial* varieties of each are simultaneously compared.

The italicized phrase "*one or more*" in the preceding paragraph embraces not only the common paired-trial procedure, with just one training trial, but also those procedures utilizing more than one training trial, as in Maier's (9) or Crannell's procedure (5). Now, a *training trial* in delayed response is any trial characterized by (*a*) sensory cues indicating the the critical response, (*b*) a constant stimulus-response relationship to 'correct' response in the test trial, for example, the same pathway previously chosen (Maier or Cowles) or the pathway not previously chosen (Crannell or Wilson, 13). It should be noted that the sensory cues referred to in (*a*) may be indicators of appropriate response through the mediation of a primitive habit (sight of food) or by a specially trained discrimination habit (brighter door), or they may be those uncontrolled stimuli responsible for the elicitation of a 'voluntary' response (in free choice of pre-delay path). With the training trial thus defined, Crannell's 'uncontrolled elimination' procedure is really *serial delayed response;* each run of the four, except the very first one, is a delayed response test in which the choice of pathway is presumably limited by retained effects of preceding responses, not by any externally imposed critical cues. The transition from delayed response in these terms to delayed alternation or even maze learning is an intriguing reflection.

With delayed response broadly defined in this manner, its importance as a logical extension of simple discrimination learning becomes more obvious. Too frequently in textbooks of general psychology or animal psychology delayed response has been relegated to a vague realm of 'higher mental processes,' which the student is led to view as isolated phenomena, more or less incapable of explanation in simpler terms. On the contrary, it is vitally related to discrimination learning and is probably as widely found among animal forms. Moreover, the mystery of what is retained over the delay period before test trial—be it a memory idea, sign-gestalt-expectation, symbolic process, or stimulus trace—is no more in need of fathoming than the same problem of what mediates retention from trial to trial in discrimination learning. It is the central

problem of all learning phenomena, and the delayed response technique promises to be extremely useful in evaluating the variables in such trial-to-trial effects. The writer has earlier listed (2, p. 126) several factors influencing delayed response which, not to mention others, need systematic investigation. (Most of these same factors are equally important in ordinary discrimination.) Hilgard and Marquis in their recent book on learning have recognized this need when they attempted a conditioning interpretation of delayed response (7, pp. 235–236). It is an encouraging sign that experimental interest in delayed response is not only increasing but is much less concerned with the determination of 'limits of delay' for various species of animals and far more concerned with the factors influencing retention.

It is quite difficult to envisage an explanatory framework for delayed response in the simple terms of an algebraic reinforcement-balance of response tendencies. It might be said, for example, that the animal's response after delay simply depends upon the relative strength of the (conditioned) tendencies to go to one goal box or the other; and, since the critical response upon the immediately previous training trial was reinforced (perhaps only by derived reinforcement or 'reward-expectancy'), therefore that response tendency is now stronger than the previously equated tendency to go to the alternative goal box. It can be seen by the figure that, after a sequence of two pairs of responses to the *same* goal box (Tests 2 and 3), that response tendency is certainly excessively reinforced in comparison with the competing tendency. However, the animal, strangely enough, seems to require only one derived reinforcement of that alternative response tendency on the next training trial (Test 4) in order to overbalance the other, presumably overweighted tendency. The problem is, then, how can the pre-delay trial lead to successful retention in spite of a recently and more frequently well-reinforced alternative response?

To account for this behavioral independence of successive tests, it may be assumed that the successful animal adapts himself to a simple alternation of adequate transfer and in-

significant transfer in the customary paired-trial procedure of delayed response. The numerous differences between the pre-delay trial and post-delay trial afford differential cues for this alternation process: the 'forced' training situation with its characteristic sensory cues for the critical response *versus* the altered, 'free choice' test situation after delay with final food reward. Furthermore, in the matter of the sequence of trials, the pre-delay response always bears a fixed relation to successful response in the post-delay trial (*e.g.*, homolateral choice), while the latter bears only a random relation to the next pre-delay response (*e.g.* 50 per cent homolateral, 50 per cent heterolateral, in a 'random' balanced series of right-left presentations). See figure. In effect, the pre-delay situation consistently facilitates subsequent transfer, while the post-delay situation remains variable, perhaps neutral, with respect to subsequent transfer.

It was in an attempt to clarify this important relation of pre-delay reinforcement and post-delay retention that the writer undertook the following experiment (3). It was assumed that what is commonly termed delayed response cannot be isolated from simple discrimination learning on the basis of absence of food reward on the pre-delay trial. The delayed response of albino rats, with the paired-trial procedure, was compared under the two conditions of food, and no food, upon the pre-delay trial, accessory conditions being practically identical in both. The results, contrary to the writer's prediction mentioned above (p. 225) and to ordinary expectation of the relative effect of actual *versus* derived reward, definitely indicated that when food is received upon the pre-delay trials retention, as measured by performance upon the post-delay trials (also food rewarded), is far *inferior* to that obtaining in a procedure of no food before delay.

Ruling out the likelihood of any constant error in our experimental technique, and without the presentation of detailed data, we might attempt an interpretation of these results. We must avoid the common mistake of treating each delayed response test (pair of trials) in isolation, for our experimental results are then clearly contradictory to the hypothesis that

better retention follows greater reinforcement strength. Any such limited treatment would also preclude fruitful application of conditioning principles, algebraic summation of response tendencies, delayed reward, or derived reward. Should the suggestion be made that our results are a clear-cut instance of the Zeigarnik uncompleted-task phenomenon,[9] we need only point to Maier's experiment and ask how by the same principle can his data be brought into relation with ours. With any of these proposed explanations some supplementary principles must be invoked, since we are not dealing with a single or continuous habit but with two competing habits each elicited at irregular intervals. In accordance with our statement above of the essential differences between so-called delayed response and ordinary discrimination learning, we might account for these results of ours as follows: The more successful animal is the animal who can behave *as if* each pair of trials *were* isolated, each test relatively free of transfer effects from any other test. The poorer success of the animals receiving food on the pre-delay run, especially if we consider our procedure of ten daily tests in rather close succession, may be partly due to the more difficult differentiation for the rat between the pre-delay situation and post-delay situation, if he is fed equally in each. A second and supporting hypothesis is that the pre-delay run with no food bears a closer behavioral relationship to the succeeding test, with a common final goal of food, than does the pre-delay run with food, which has a final goal of its own like every other run in that procedure. These hypotheses can readily be tested by further delayed response experiments.

Our purpose in considering this experiment, even superficially, is to illustrate the problem of pre-delay reinforcement which is encountered in delayed response experiments and which can undoubtedly be brought toward solution by that very technique. The discussion should emphasize the futility

[9] See Tolman (12, p. 157, footnote 9) for such a suggestion, which is entirely consistent with his particular delimitation of delayed response. His sign-gestalt treatment (*ibid.*, pp. 143–163) is probably the earliest systematic interpretation of delayed response, and certainly is one of the few which have called attention to its relationship to other learning situations.

of endeavoring to describe delayed response with regard only to what occurs within a single test removed from its matrix of successive tests. We also hope to have contributed in this paper to the integration of delayed response with other learning processes, particularly discrimination learning. In daily life we rarely retain discriminations in relative isolation, and any technique which can pose or answer questions concerning the elicitation, interaction, and retention of competing discriminations is worthy of experimental exploitation. Delayed response is not an uncommon phenomenon but a fundamental behavioral process, often misunderstood but more often ignored.

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PSYCHOPHYSICS AND MENTAL TEST THEORY. II. THE CONSTANT PROCESS

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In a previous paper the writer presented the fundamental relationships which obtain between the field of mental test theory and the methods of psychophysics, or rather, the psychophysical processes (2). There it was pointed out that both fields derived their theorems from a basic set of undefined terms and relationships, definitions and postulates, and that the assumptions peculiar to each field were mathematically transposable. The terms, *individual*, *stimulus*, *response*, are undefined in both fields. The fundamental relationship is: An individual responds to a situation (4).

The basic data in either field consist of the responses, R, of an individual, i, to a situation (stimulus), j, and these data can be arranged in the form of a matrix:

$$(1) \qquad R = \begin{vmatrix} R_{11} & \cdots & R_{1j} & \cdots & R_{1n} \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ R_{i1} & \cdots & R_{ij} & \cdots & R_{in} \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ R_{N1} & \cdots & R_{Nj} & \cdots & R_{Nn} \end{vmatrix}$$

*(2)[1] It is assumed that the response R_{ij} can be expressed in additive numbers either directly, or by means of some intervening function.

The response, R_{ij} is considered as consisting of two components, such that

$$(3) \qquad R_{ij} = x_{ij} + e_{ij}$$

or, in more general form,

$$(4) \qquad R_{ij} = F(x_{ij} + e_{ij}),$$

[1] Propositions which constitute assumptions will be preceded by an asterisk.

where x_{ij} is defined as the same for successive responses of the same individual to the same stimulus, where the two presentations differ infinitesmally in time, whereas e_{ij} is defined as that part or component of the response which will differ from presentation to presentation of the same situation to the same individual. This can be expressed symbolically as:

(5) $$x_{ij} = x'_{ij},$$

where x' represents a second presentation in time.

(6) $$e_{ij} \neq e'_{ij}.$$

A further assumption, basic to both fields, is that x_{ij} and e_{ij} can be expressed on the same linear continuum, *i.e.*,

*(7) $$e_{ij} \equiv \Delta x_{ij}.$$

It might be well to summarize here the notation.

R	represents a response
$h, i,$	are subscripts denoting individuals, of whom there are N
$j, k,$	are subscripts denoting situations, of which there are n
R_{ij}	is the response of the i-th individual to the j-th stimulus
x_{ij}	is the constant component, or 'true value' of R_{ij}, as defined by the equations (3) and (5)
e_{ij}	is the variable component, or 'chance error' of R_{ij}, as defined by equations (3) and (6)
s_j	is the value of the j-th stimulus, in units appropriate to that physical modality. This concept will be discussed in more detail later.

In the light of equation (3), the matrix represented in (1) may be considered as the sum of two matrices, a matrix with entries x_{ij}, and another, the error matrix, with entries e_{ij}. In these matrices, as in the matrix R_{ij}, a single row represents the responses of a single individual to the range of situations, a column represents the responses of the group of individuals to a single stimulus.

These constitute the postulates and definitions common to the two systems of test theory and psychophysics. From this point on, postulates, definitions and deductions are specific to the one field or the other, but for each one in the field of psy-

chophysics, its transpose may be stated in the field of mental test theory by transposing the relations of i and j.

Certain other concepts presented in the previous paper should be reviewed here. The first postulate of psychophysics is that there exists for each stimulus its characteristic response value, the same for all individuals of a certain class—*e.g.*, not color blind—and depending only on the stimulus situation (within the universe of individuals under consideration). In terms of symbols:

$$*(8) \qquad x_{ij} = x_{hj} \equiv x_j.$$

This assumption is equivalent to assuming that every value within any column of the matrix x_{ij} is constant.

For the field of mental test theory one may state the transpose assumption that there exists for each individual a characteristic response value, the same for all stimuli of a certain class, and depending only on the individual (within the universe of stimuli under discussion), *i.e.*,

$$*(9) \qquad x_{ij} = x_{ik} \equiv x_i.$$

This assumption is equivalent to assuming that every value in any row of the matrix x_{ij} is constant.

The mean of any column of the error matrix, e_{ij}, is defined as $\bar{e}_j$, and its transpose, the mean of any row of the error matrix is defined as $\bar{e}_i$. It is assumed that:

$$*(10) \qquad \lim_{N\to\infty} \bar{e}_j = \lim_{N\to\infty} \bar{e}_i = 0,$$

i.e., that e_{ij} are random variables, whose expected or mean values are zero. The transposable concepts, ϵ_j^2 and ϵ_i^2, are defined as the standard deviations of any column and any row, respectively, of the error matrix.

By the use of these definitions and postulates, the method of equal-appearing intervals was derived in its application to psychophysics and to the problem of test scoring when responses R_{ij} are quantitative and continuous over a selected range of values. Similarly the Law of Comparative Judgment was derived in its customary psychometric setting and extended to the problem of scaling individuals (4).

Before taking up the special problems which constitute the main consideration of this paper, a distinction should here be made which was implicit in the previous paper, but not explicitly formulated, and which might lead to confusion. This is the distinction between the psychophysical methods as experimental procedures, or designs for experiment, psychophysical processes (2) as the quantitative rationale which deals with the treatment of the data resulting from a particular experimental set-up, and psychophysics as the cumulated body of generalizations resulting from the application of methods and processes to specific problems, *e.g.*, Weber's Law. The development here is concerned only with the rational, quantitative aspect of the psychophysical processes, including the psychological scaling methods.

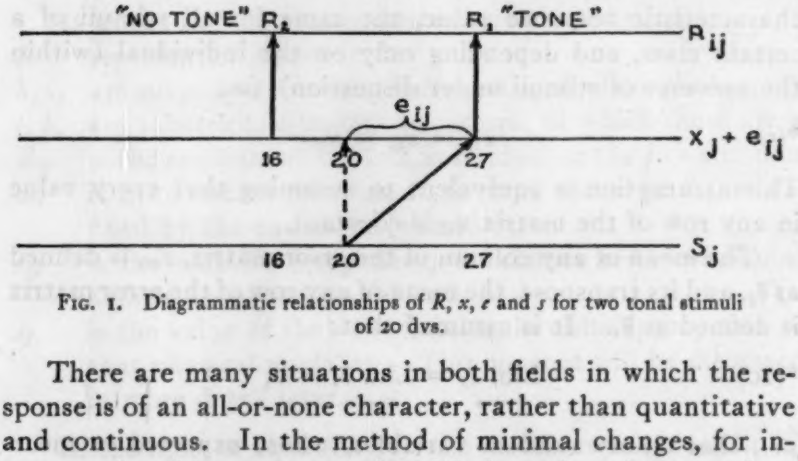

FIG. 1. Diagrammatic relationships of R, x, e and s for two tonal stimuli of 20 dvs.

There are many situations in both fields in which the response is of an all-or-none character, rather than quantitative and continuous. In the method of minimal changes, for instance, the observer responds with 'tone,' or 'no tone,' or 'same' or 'different.' In the most common forms of mental tests, items are scored 'right' or 'wrong.' It is of importance to apply the present rationale to these cases also, in order that the techniques for handling these situations which have been developed in one field may be applied to the other whenever such application would appear fruitful.

To apply this analysis to a concrete situation, let us consider the physical continuum of frequency of vibration of sound waves. This is represented in Fig. 1 by the line s_j

(the stimulus). We can assume that any stimulus, say 20 dvs., arouses a characteristic change in the individual which is the same from time to time ($x_j = x'_j$), but which is attended by varying chance errors, due to unknown changes in the conditions of stimulation, or in the psycho-physiological condition of the responding individual. These chance errors differ from one presentation of the stimulus of 20 dvs. to another. The values x_j and e_{ij} are defined here as quantitative values along a hypothetical continuum, which contains that function of the responses which can be expressed in additive numbers. Associated with each value of $x_j + e_{ij}$ is an observable, response, R_{ij}, which may or may not be quantitative. In this instance the response might be either the words 'Tone,' or the words 'No tone.' Thus in the example, a sound of 20 dvs. is presented to a subject on Tuesday and arouses the psychological process $x_j + e_{ij}$. If e_{ij} on Tuesday is $+ 7$, then $x_j + e_{ij}$ is 27. Note that the stimulus, s_j, is not 27, but 20. Rather, the psychological process $x_j + e_{ij}$ which is aroused on this particular Tuesday is the one which is usually aroused by an s-value of 27 and which we identify by that number. However, the individual who has been stimulated by $s_j = 20$, in whom the psychological process, $x_j + e_{ij} = 27$ has been aroused, does not respond with the quantitative, additive number 27, but with the non-additive, but observable, response, 'faint tone.' Note that on Wednesday (or one minute later on Tuesday), the same s_j will arouse the same psychological process, x_j, which may be accompanied by a different chance error, e'_{ij}, say $- 4$, leading to a composite process $x_j + e'_{ij}$ of 16, to which he makes the observable response, "I don't hear anything." The problem is to go from the non-additive, but observable responses of 'Faint tone' and "I don't hear anything now" to the values of the psychological process x_j which are unobservable, but additive.

If we choose to assume that the individual reacts characteristically, say with a given degree of intelligence, to all situations of a certain type, but the characteristic values vary from individual to individual, we may transpose the subscripts i and j and utilize the same figure. Thus if an indi-

vidual characteristically reacts with a degree of intelligence x_i, and is presented with a situation which requires the intelligence of a seven-year-old, the psychological process $x_i + e_{ij}$ which is aroused will result in an observable response which is right or wrong depending on whether the composite process of characteristic degree of intelligence plus the chance error is greater or less than the intelligence characteristic of the class of seven-year-olds.

The problem here can be stated as that of determining the value of x_i or x_j when R_{ij} is not multi-valued but can take only one of two possible values. The psychophysical processes under consideration here, *e.g.*, the method of minimal changes and the constant methods, depend primarily on the fact that, while the response is not multi-valued and continuous, the stimuli can be so arranged. This leads to the necessity of the following assumption, basic to further development of the problem in both fields:

*(11) s_j, s_k, can be ranged on a quantitative continuum; $s_{j+1} > s_j$.

It is then possible to define the response for an individual in terms of the situation which modally evokes it. The analysis of the response, R_{ij}, into its two components, x_{ij} and e_{ij}, in equation (3) must be replaced by the more general form of equation (4):

(4) $$R_{ij} = F(x_{ij} + e_{ij}),$$

where

(12) $$F(x_{ij} + e_{ij}) = A \quad \text{for} \quad (x_{ij} + e_{ij} > s_m)$$

and

(13) $$F(x_{ij} + e_{ij}) = B \quad \text{for} \quad (x_{ij} + e_{ij} < s_m).$$

A and B in the two equations above correspond to the responses judged 'correct' and 'wrong' respectively in the mental test situation, or to the responses 'tone' and 'no tone' in the constant method, and s_m is the critical point on the stimulus continuum which differentiates the two responses.

In the psychophysical situation, where equation *(8) is assumed, the value x_j is defined as equal to s_j, but the value of

fundamental interest is $x_m = s_m$. In the mental test situation, where assumption *(9) is made, x_i is independent of the stimulus magnitude, and may be defined directly as equal to s_m. It should be noticed that the deviation from strict transposability in the selection of the stimulus range as the defining unit in both situations here (rather than agreeing to define the x_j values in terms of s_j, the x_i values in terms of 'units of the individual,' whatever that might mean) results in a corresponding departure from transposability[2] in the definitions of the values of x_i and x_j. The characteristic response values for each stimulus are measured in units of the stimulus, and differ for each stimulus; only one of the x_j's is of unique interest, the critical value x_m corresponding to s_m. The characteristic response values for the individual, however, are the same for all stimuli, s_j, and the characteristic value is uniquely $x_i = s_m$.

It will be convenient to make these definitions of x_j and x_i more explicit for ready reference:

(14) $x_{hj} = x_{ij} \equiv x_j \equiv s_j$ when proposition *(8) is assumed.

Each stimulus is presumed to yield a characteristic response value, x_j for all individuals, h, i, and this response value is measured in units of s_j. Only the value of $x_m = s_m$ is unique with respect to R_{ij} and can be determined. For the mental test situation:

$$x_{ij} = x_{ik} \equiv x_i \equiv s_m. \tag{15}$$

Each individual is assumed to have a characteristic response value, x_i, which is the same for all stimuli, can be measured in units of, and defined as equal to, s_m of propositions (12) and (13).

Applying the analysis once more to concrete situations for clarity, we recall that in the problem of the two-point thresh-

[2] If the individuals, or groups of individuals, can be ranged in order comparable to the ordered continuum s_j, strict transposability can be maintained. Thus a degree of intelligence is defined in terms of that class of individuals characteristically exhibiting it, just as a tonal sensation is defined in terms of the stimulus or class of stimuli which characteristically evoke it. This leads to the age-scaling methods and is discussed elsewhere. It should be noted, however, that the defining class of individuals is specified in physical units to which have been assigned psychological significance, *e.g.*, age.

old, the threshold is assumed to be the same for all observations treated together *(8). This 'true' value is affected by chance errors (1). When the response to a stimulus (including both the response characteristic of the stimulus and the associated chance error) is greater than the threshold, the response is 'two-point' (12), when it is less, the response is 'one-point' (13). The value of the threshold, x_m, *(8), is expressed as a stimulus-distance in units of millimeters—s_m of equations (12) and (13).

In the case of intelligence tests, *e.g.*, the Stanford-Binet, the stimulus situations are assumed to have been, or to be capable of being, scaled in order of the age at which the item is characteristically answered (11). When the intelligence of the individual, x_i, plus the chance error component of his response, e_{ij}, is greater than the level of intelligence necessary to answer correctly (s_m), he answers correctly (12); when it is less, he answers incorrectly (13), and his intelligence is reported as that stimulus value, s_m, or age, which marks the division between 'right' and 'wrong' answers (15).

From this point on it will be convenient to carry through the development for the psychophysical situation and recur later to the mental test problem. If the factor e_{ij} did not enter into equations (12) and (13), the situation could be represented by the following diagram. The situations, $s_1, s_2, \cdots s_j \cdots s_m \cdots s_n$ are arranged along the base line in ascending order of some quantitative attribute, let us say wave frequency (assuming auditory stimuli). The ordinate gives the value of R_{ij}, which can take only the values A or B, *e.g.*, 'tone,' 'no tone.' It is assumed that all responses are made without error.

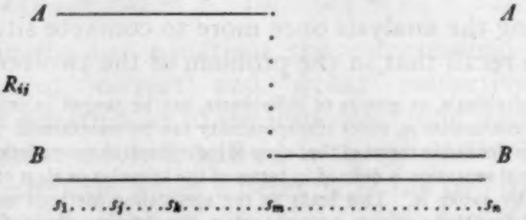

FIG. 2. Response values to each stimulus value, assuming all errors equal to zero.

If, instead of a single application of each stimulus, we are dealing with a large number of responses to each stimulus, then we may take as the ordinate, the value $P_{R=A}$, the proportion of A-responses for stimulus s_j. If, again, the responses R_{ij} are without error, the figure is:

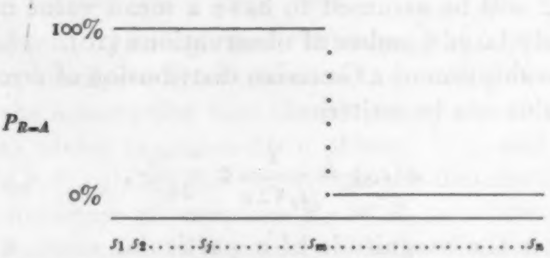

FIG. 3. Proportion of A-Responses to each stimulus value, assuming all errors zero.

It may be objected that Fig. 3 is unrealistic. The figure admittedly represents an hypothetical situation, greatly oversimplified. If, for any reason, *e.g.*, practice effects, the drop from 100 per cent to 0 per cent does not occur at a single value of s there are two alternatives. A single, typical value of s_m (the mean) may be taken as the true value of x_i, and the discrepancies between each observation and this assumed true value treated as chance error (assumed to be zero in the figure). If this treatment is not possible, then the basic assumption *(8), that there exists a value, x_i, characteristic of the individual for all of the observations under consideration, is not met, and the entire discussion does not apply. It is precisely to emphasize that unless such a single characteristic value, x_i, can be assumed to exist, essentially the same for a single individual for the range of observations considered, measurement is impossible, that this and the preceding article have been designed.

The diagrams of Figs. 2 and 3 apply either to the psychophysical situation or to the mental test situation, according as it is x_j or x_i which is assumed to be equal and equal to s_m. We do not, however, find responses uncomplicated by error, and the theoretical picture must be altered to account for errors in the responses as well as for the true values. In the

previous article (4) it was seen to be reasonable to assume that the distribution of errors, either for a particular stimulus or for a particular individual was Gaussian (1). The errors, like the x-values, will in this situation be measured in the units of the quantitative physical continuum, *e.g.*, millimeters, or age-years, and will be assumed to have a mean value of zero for an infinitely large number of observations (10).

The assumption of a Gaussian distribution of errors for the j-th stimulus can be written:

$$*(16) \qquad \phi(e_{ij}) = \frac{1}{\epsilon_j\sqrt{2\pi}} \mathrm{e}^{\frac{-e_{ij}^2}{2\epsilon_j^2}},$$

where e_{ij} is the magnitude of a particular error, e (without subscript) is the base of the natural logarithms, ϵ_j is, as defined previously, the standard deviation of the distribution of errors, e_j, for the j-th column of the error matrix. A similar function could be written by replacing j by i for the distribution of errors of a particular individual to a series of stimuli.

Returning to equations (12) and (13), we can restate the condition that $R_{ij} = A$, *i.e.*, that the response of the i-th individual to the j-th situation will be 'right,' or 'greater,' or 'tone,' or 'two-point,' depending on the experimental situation and the definitions of response A. Restated, this condition becomes, for the assumption of psychophysics *(8).

$$(17) \qquad e_{ij} \geqslant s_m - x_j, \qquad R_{ij} = A.$$

Similarly, the condition for $R_{ij} = B$ is restated as

$$(18) \qquad e_{ij} < s_m - x_j, \qquad R_{ij} = B.$$

But $x_j = s_j$, by definition of units of x_j (14), and s_m is a constant by *(8) and (14). We can represent the probability of the event in the condition (17) for every x_j by the integral of (16), where e_{ij} is measured in the same units as $s_m - s_j$.

$$(18a) \qquad Pe_{ij} > s_m - x_j = \frac{1}{\sqrt{2\pi\epsilon_j^2}} \int_{\frac{s_m - s_j}{\epsilon_j}}^{\infty} \mathrm{e}^{\frac{-e_{ij}^2}{\epsilon_j^2}} de_{ij}.$$

If, of course, ϵ_j^2 varies irregularly as a function of s_j, then the resulting curve becomes an irregular deviation from the Phi-

Gamma function. Under these conditions, the problem of determination of x_m and ϵ_j^2 becomes insoluble. If, however, we assume that within the range of stimuli being considered, $s_1 \cdots s_j \cdots s_m \cdots s_n$,

$$*(19) \qquad \epsilon_j^2 = \epsilon_m^2,$$

we are reduced to a single error function which can be represented graphically on the same coordinates as those of Fig. 3.

With the assumption that the frequency distribution of e_{ij} is Gaussian about the mean error of zero, (16), and that ϵ_j^2 is equal to ϵ_m^2, *(19), we can show that the distribution of R_A, *i.e.*, the proportion of responses $R_{ij} = A$, as a function of the stimulus magnitudes, is identical with the distribution of e_{ij}, and hence the value of s_j for which $s_m - s_j = 0$ (*i.e.*, the value of s_m and thus x_m) as well as the value of ϵ_m^2, can be determined from data. The probability that R_{ij} is A is given by two factors; the first of these is due to x_{ij} alone, which is either 1.00 or zero, as j is less than, or greater than m (*cf.* fig. 3); the second is the probability that e_{ij} is greater than $s_m - s_j$. The desired probability of R_A is the sum of these factors. The derivation may be most conveniently carried out in two steps, for j less than m, and for j greater than m.

For j less than m,

$$(20) \qquad P_{(R_{ij}=A)} = 1.00 - P(e_{ij} < s_m - s_j).$$

However, since ϵ_j^2 may differ for every s_j (*e.g.*, if Weber's Law or any other similar formulation holds), the probability will have to be computed from a separate Phi-function for each s_j. This is the point made by Thurstone in his discussion of the Phi-Gamma Hypothesis (5). Consideration of an actual example will make this clearer. Let us consider an hypothetical example as shown on page 246.

In the example the threshold value, s_m, is taken as 20. The first group of columns through column 5 presents the results for a situation in which ϵ_j^2 changes radically, but regularly with each x_j. In the right hand group of columns, columns 6–8, ϵ_j^2 is assumed to be constant at the value of ϵ_m^2 and the data presented for this situation. It will be noted that

DATA IN AN HYPOTHETICAL DISCRIMINATION PROBLEM

1	2	3	4	5	6	7	8
s_m	x_j	e_j^2	$\frac{s_m - x_j}{e_j}$	P	e_j^2	$\frac{s_m - x_j}{e_j}$	P
20	16	2	2.00	.95	6	.67	.75
20	17	3	1.00	.84	6	.50	.69
20	18	4	.50	.69	6	.33	.63
20	19	5	.20	.58	6	.17	.57
20	20	6	.00	.50	6	.00	.50
20	21	7	−.14	.45	6	−.17	.43
20	22	8	−.25	.40	6	−.33	.37
20	23	9	−.33	.37	6	−.50	.31
20	24	10	−.40	.35	6	−.67	.25

Thurstone's conclusions are strikingly verified. The two curves agree in the determination of s_m, but the curve with increasing e_j^2 is markedly skewed. This would correspond to the situation of Weber's Law where Weber's fraction was 1.00, an extreme case, but one which serves to point the distinction between the two conditions. For j greater than m,

$$P_{(R_{ij}=A)} = 0.00 + P_{(e \geq s_m - s_j)}. \tag{21}$$

But if we note that

$$P_{(e < s_m - s_j)} = 1.00 - P_{(e \geq s_m - s_j)}, \tag{22}$$

then equations (20) and (22) may be combined to prove our theorem

$$P_{(R_{ij}=A)} = P_{(e_{ij} \geq s_j - s_m)}. \tag{23}$$

This is illustrated graphically in Fig. 4.

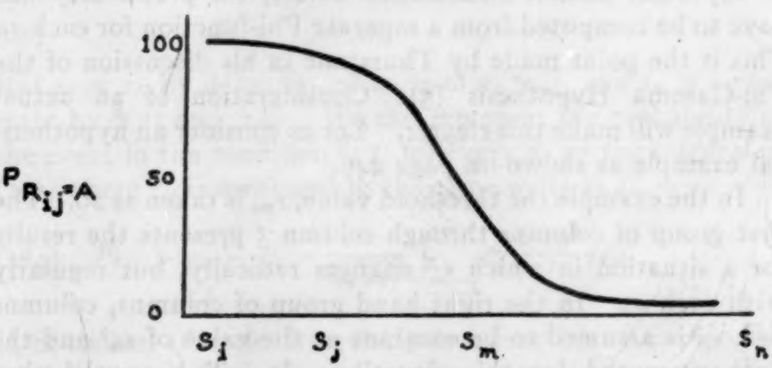

FIG. 4. Probability of $R_{ij}=A$ as a function of s_j.

It will be well to remind the reader at this point that the abscissae of Figs. 2–4 refer to any stimulus magnitude, arranged in a quantitative continuum according to some attribute—intensity of a light, length of a line, the distance between two points of an aesthesiometer, or the difference between two weights. In the mental test situation, to which Figs. 2–4 also apply, provided assumption *(9) holds, the s-continuum will most typically apply to items arranged in order of difficulty, scaled in terms of proportion of a standard group passing the items, or scaled in terms of the age at which the average child passes the item. It will be noted that the method of definition of the scale of e_{ij} as equal to $s_m - s_j$ has the effect of making the errors to the left of s_m (as shown in Fig. 3) positive, and those to the right of s_m negative. Thus if the sense of the physical continuum be the conventional one, with the larger stimulus magnitudes to the right, the sense of the error continuum is reversed.

The foregoing laborious derivation of the constant process has not been with the idea that it was new, but to bring this important psychophysical process within the framework of the postulational system, showing how it can be derived from the postulates and definitions, to emphasize the generality of the process apart from its experimental setting, and to demonstrate the mode of deriving the transpose theorem for mental test theory.

The transpose theorem for mental test theory may be stated formally as follows: When stimuli, including stimulus differences (*e.g.*, right answers and distractors) are arranged in order of stimulus magnitude along a quantitative continuum, but the response of any individual to any stimulus can take only one of two 'values,' then the unit frequency function of response as a function of stimulus magnitude will be the integral of the normal probability curve. Then also the characteristic response value of an individual (his test score) is that stimulus magnitude yielding each of the two possible responses 50 per cent of the time, and the individual's variability is the standard deviation of the probability function thus integrated.

Restated particularly for the mental test situation, the theorem becomes: *When an individual is presented with a series of test items varying in difficulty, and his response is either 'right' or 'wrong,' then the distribution of the proportion of correct answers will be the integral of the normal probability curve; the individual's test score will be that difficulty value which is answered correctly 50 per cent of the time, and the individual's variability will be the standard deviation of the probability function whose integral is the proportion of 'correct' answers as a function of difficulty.* The theorem depends on the same assumptions previously made, stated explicitly for *(9).

The first part of the theorem relating to the determination of test score has been stated by Thurstone (6) although he does not point out its relationship to the constant process, and he does not there point out the implications for test theory of the standard deviation of the distribution as a measure of individual variability. Hertzman has made use of the concept of individual variability, derived somewhat differently (3).

The writer has applied the foregoing theorem to the problem of scoring tests by ranging the items in order of difficulty, recording the proportion of correct answers as a function of difficulty and applying the constant process to the resulting data to determine for each individual his difficulty of median error, x_i, and his dispersion, ϵ_i^2. Although an inadequate number of items and only graphic methods of determining x_i and ϵ_i^2 were used, the corrected reliability of x_i based on odd-even items was .88 compared with a value of .94 for the 'number-right' scoring. The reliability of ϵ_i^2 was disappointingly low, .55, a value still significantly different from zero. The correlation between the 'constant' method and the 'number-right' method, corrected for attenuation, was 1.016, indicating that the more convenient and more reliable 'number-right' method may be substituted for the logically more justifiable constant method, except for the determination of ϵ_i^2.

In the foregoing, the postulational system previously proposed for the unification of the fields of psychophysics and mental test theory (4) has been extended to cover the in-

stances of two-valued responses. The constant process of psychophysics has been derived from the definitions and assumptions and an analogous method developed for scoring tests. In the development, Thurstone's age-scaling methods (6) and his strictures on the Phi-Gamma Hypothesis (5) have been related to the present framework. The relationship between the 'constant method of test scoring' and scoring by the customary 'number-right' method has been indicated. The importance of the concept of individual variability in test theory, analogous to the precision-values of psychophysics, has been emphasized. The results of applying these concepts to data have been briefly treated.

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ON THE MEANING OF INTELLIGENCE [1]

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I

It is well known that Binet developed his scale for the measurement of intelligence in response to a demand for the early discovery of children likely to fail in school. Improvement of the tests over the years was a somewhat desultory process. Theory was kept to a minimum, the tests being constructed around the simple principle that a large number of graded small tasks, each involving the higher thought processes, would approach a concept of *general mental ability*. It was clear that the items could be arranged in ascending clusters, conforming crudely to chronological age. Children readily accepted them as goals.

There has been no change in the underlying concepts of the most widely used American revisions of the Binet-Simon scales, unless the utilization of Stern's intelligence quotient could be so regarded. Certainly this quotient has done much to bring mental testing before the scientific world and the public in general. Its ease of computation and its ready application in school or clinic have indeed lulled many a worker into the feeling that now we have something mathematical and fixed, something comparable to the measures and pointer readings of our friends in the physical sciences.

In reality the IQ concept is not simple. Let us take a quick look at the numerator, or mental age, as determined, let us say, by the 1937 Stanford Revision. Since at this point no controversial issues are involved, time can be saved by a simple enumeration of the considerations given weight in the selection of the test-items:

[1] Presented at the meeting of the American Psychological Association, Pennsylvania State College, September 4, 1940.

(1) increase in percentage passing with increase in chronological age
(2) a correlation of the per cent passing an item with mental ages from the 1916 revision, or with a composite total score
(3) ease of administration and scoring
(4) appeal to the child
(5) brevity
(6) lack of sex imbalance.

All these variously employed and pragmatically weighted criteria were taken in conjunction with the tacit understanding that, for a sampling of American-born white children, each test and the composite should be related to pupil achievement in standard subject matter. They do not, however, throw much light on the meaning of intelligence, for we come out with the old tests familiar to Binet or to the Army workers: opposites, comprehension, analogies, vocabulary, similarities and differences, completions, absurdities, memorizing, etc. In any such omnibus, the 'ability to do abstract reasoning,' like Spearman's 'g,' is carried more like a spare tire than a paying passenger! What the primary mental abilities involved are, I do not know, but one could expect to find Thurstone's perceptual, verbal and memory factors. Such patterns, as they are determined, will depend to some extent upon the chronological and mental age of the subjects.

Under the conditions cited above, it was possible, for a small sampling of children, to make the mean mental age equal to the mean chronological age. This would yield average IQ's of 100, for the best range of the testing (the elementary school years), but would not guarantee a constant IQ for any child. Rather, for children somewhat removed from the mean mental ability, differences in variability up through the years have always led to marked variations in IQ. These unequal distances away from the mean for the same IQ at different age levels, together with the fact that the scales are not standardized on adults, even though applied to them, lead to obvious complications.

Certainly a mental age of fifteen is no more meaningful for an average thirty-year-old than a mental age of thirty for an average fifteen-year-old. If it is helpful to say that a six-year-old is bright, knowing full well that his mental maturity is at a low level, then we can make the analogous statement about a sixty-year-old. We do not say, "the child is bright *for a six-year-old,*" since that is redundant. Neither should we say that a man is bright (or dull) for sixty. That, too, is redundant. Either he is bright or he is not, his age being utilized as a reference point for entering the tabulation of scores. For those still older, to be bright is to postpone senility.

Analysis of the curves of mental growth is legitimate, but it should not depend primarily on IQ's. However, successively determined IQ's, based on valid and standardized materials, could furnish a plat of a person's changing, or unchanging, intellectual status in respect to his chronological peers.

It is to be noted that in no sense does the validity of an IQ depend upon its fixity. Only if a child maintains his relative status should the IQ remain constant; to find it constant otherwise would be a mark of insensitivity and hence invalidity in the test.

II

To go into practical aspects of mental testing, however, is not my purpose. The question is, what *is* the nature of mental ability, of intelligence, as revealed by measurement? Unfortunately no answer can be given that will satisfy persons interested in child and adult development, for the subtests are not revealing. On the other hand, if you want to measure memory, reading comprehension, reasoning ability, vocabulary and general information, good procedures are available. Almost any combination of reliable measurements of these functions, accompanied by clinical observations, will give fair predictions of academic success, and of general ability to solve problems.

It has become painfully clear that without the aid of the clinic, the use of IQ's by teachers, parents, physicians or

judges leads to unhappy results. The same conditions that lead to poor school work may yield low IQ's; in fact, there is thus far no higher validity for mental testing than its tendency to parallel school achievement. Under cultural conditions that make school work dominant from ages six to eighteen, and highly important at younger and older ages, this is to be expected. For the child, the safe, prudent, approved life involves, above all, a steady familiarity with the three R's and their up-to-date companions.

In brief, the meaning of intelligence, as it emerges from all child testing Binet in type, is *scholastic aptitude*.

To say this is not to deny the existence of a tremendous span of intelligence from idiocy to genius. There are persons so organically defective that one could not think of a free competition in which they could succeed; and others so strong as to prosper under many, but not all, social conditions. But our present tests are poorly adapted for exploring intelligence over this great range.

III

If one were, in all candor, to undertake the measurement of intelligence, it would seem helpful to define the term and then to set up the necessary operations and test procedures. Confusion has resulted from the long failure to proceed in this manner. Even errors or defects in the definition would not be as bad as what we now have, for a worker could always say, with some truth, that *if* you define intelligence in such and such a way, then here is a test of that function which meets certain criteria of reliability and validity.

For example, we could define intelligence as the ability to do good work in school. It could be estimated solely from measured achievement. As I have indicated above, this is really not a far-fetched definition, testing being what it is. But, one objects, a child might get ill or neurotic or bored, thus failing. No matter; that is provided for in the definition. He ought to be better, or know better, or do better—if he is to be judged *intelligent*.

We shall never arrive at any clarity if we allow *causes* for variation to be confused with the concept under consideration.

Thus *running ability* can be well measured under standard conditions. Among those with low running ability quotients would be found the lame, the fat, the ill, the decrepit, the untrained, the uninterested. But we must stick to our measurement guns—there is no point in estimating how they could have run, shorn of these disabilities. That may be the coach's job, but the tester is the timer. Similarly, it would be a theoretical and practical advance, if mental abilities could be analyzed and combined in conformance to some clear ideas of what we are trying to do.

There have been some mild attempts to do this in behalf of Spearman's principle of the eduction of relations, and the recent work of Wechsler on the Bellevue Intelligence Tests moves in this direction. The second stage of the factorial analysis of numerous mental tests illustrates the principle, difficult as it may be in actual practice. Having identified certain abilities designated primary, which may turn out to be structure-like organizations of mental processes and are at least somewhat independent clusters of abilities, Thurstone has taken the next step, namely the construction of test materials designed to measure these abilities. Whether or not it leads immediately to the improvement of scholastic predictions, at least it is promising as a potential revelation of mental organization. If you know what you are looking for, you will find it in unexpected places.

IV

Accordingly let us pay some attention to definition. During the past few years I have brought together what impress me as the principal attributes of a functional concept of intelligence. Most of the separate portions are not original, but current in psychology from Binet to Thorndike. The composite is new, at least in the sense that nobody else is likely to lay claim to it! It is based not on factors, components or special abilities, but on qualities. Each attribute springs from the whole functioning organism, but the main focus is intellectual.

Intelligence is the ability to undertake activities that are characterized by (1) difficulty, (2) complexity, (3) abstractness, (4) economy, (5) adaptiveness to a goal, (6) social value, and (7) the emergence of originals, and to maintain such activities under conditions that demand a concentration of energy and a resistance to emotional forces.[2]

As Thurstone says of a primary ability, a concept of intelligence is futile unless it reduces and simplifies. At first glance, this formulation may appear to be as big as a bus off the main highway. What do these terms mean? Let us take them up, with just a phrase for each:

(1) *Difficulty* is a function of the percentage passing. Throughout any series of mental measurements, it must increase with chronological age, so long as we postulate mental growth.

(2) *Complexity* refers to the breadth or area: not only how difficult the task, but how many kinds of tasks may be successfully undertaken? Attributes (1) and (2) are related in the sense that high accomplishment is pyramidal in structure.

(3) *Abstractness* is a means of connecting mental ability to symbolic relationships. It eliminates, as such, physical and motor acts.

(4) *Economy* is another name for speed—the accomplishment of the most mental tasks in the least time. It calls for early good choices, for faster insights.

(5) *Adaptiveness to a goal.*—It is not enough to perform speedily difficult and complex tasks. There must be a goal, a purpose as against aimlessness, and plasticity as against rigidity.

(6) *Social value* as an attribute is useful in keeping intelligent actions within the normal range of human behavior. Thus insanity is not, from this standpoint, something that the human mind may endure without loss of intelligence.

[2] For an earlier form of this definition see Stoddard and Wellman, *Child psychology*, New York: Macmillan, 1934, p. 176.

(7) *The emergence of originals* is included because of its special place at the upper end of any valid distribution of intelligence. It is characteristic of genius. While related to high ratings in the six preceding qualities, it is not an inevitable outcome of such ratings.

No longer are IQ's of 140 or above regarded as of 'genius' level. The tendency is to place the lower limit much higher, perhaps at 170. But this will not suffice either, for the good reason that Binet testing offers little opportunity to show what new solutions or original patterns a child can produce. Binet tests, by definition, are restricted to the simple, the piece-meal, the common, the overlearned.

The next step is to place children and adults along a continuum in which a low composite score in these attributes means low intelligence, and similarly up to the appearance and development of original ideas. Until recently it has appeared quite satisfying to rest at this point, with the thought that a new intelligence scale could follow these lines. With certain improvements, learning, vocabulary, comprehension and contractual tests could act as a vehicle carrying most of the indicated measurement load.

But now I must insert two conditions, without which it is difficult to explain why so many 'intellectual' persons can be so habitually unintelligent. (These persons would do well on mental tests saturated with the demands listed above, with the probable exception of numbers six and seven.) The two conditions are a concentration of energy and a resistance to intrusive emotional blockings or distortions.

The first condition postulates a selecting, rejecting, recombining process moving in the direction of larger goals; sheer accumulation of information is the antithesis of intelligent activity. Intelligence, feeding upon an amorphous mass or an endless aggregation of facts, bogs down. It begins to look strangely like the 'standard' school curriculum which, by overwhelming the pupil, teaches him to accept a half knowledge about a thousand things as superior to a full and clear understanding of certain basic principles in human knowledge

and behavior. It enshrines half-learned, soon-forgotten details while neglecting the few dozen basic principles in the physical, biological and social sciences that could forever inform and delight the inquiring mind. It is keen on *knowledge about*, and cool toward insight and participation.

Out of school, the failure to grasp the real significance of *intelligent* behavior leads to a wallowing in 'mental stunts,' to cross-word puzzles, to question-and-answer superficiality. If such activities are carried to extremes, that is to say, beyond their preserves in harmless pastime, the whole effect is to make a wastebasket of the human mind. It is not in facts *per se*, nor in the first person, place or chronology that comes to mind, but in their relationship to generalized principles and to goals in human development and behavior, that order appears out of chaos.

In a world that, in and out of school, has developed contact and communication to a marvelous degree, this attribute of intelligence appears crucial. It is a guard against mental indigestion. Perhaps it can be framed as a question: How many hours has this person devoted to the systematic exploration of any mental question? That such expenditure of energy is related to motivation, is obvious. It is likely that the distinctive analyses of Kurt Lewin could throw light on its relative strengths among different individuals.

This leads directly to the second condition in the concept of intelligence. While emotional forces are excellent in their time and place, they play havoc with the decisions and actions that depend upon logic and objective relationships. If 2 and 2 are ever to be anything but 4, the change must be brought about by mathematicians, not by ignorant or neurotic persons who dislike 4.

The whole question of propaganda is a case in point. The propagandist conceals his intentions and tries to establish relationships in your mind that could not be tolerated if all were known. This is done presumably for your welfare. In other words, you are not to be trusted with full knowledge, for you might come to the wrong (that is, right) conclusions. It is a mental legerdemain employing labels, symbols, dark

rooms, and favored gestures and traditions. It is not necessarily a result of international crises, although these may widen the scope and increase the deadliness. Since emotion-arousing labels are a stock-in-trade, any person, other things equal, is intelligent to the extent that he remains undeceived.

V

Thus far I have said little about the form of testing required to carry the load of such mental attributes.

Since we know that adults are more differentiated than children, expressing their intelligence in greater penetration into certain areas (such as those represented in vocations and professions), it is desirable to permit adults to make choices. In addition to a general rating, in what areas are they most at home mentally? A follow-up testing in those areas would permit greater variability among those previously judged equal and would raise the test ceiling.

So far as emotional resistance is concerned, the methods developed in recent studies of propaganda are promising. They could be adapted to a scale of intelligence. The energy-concentration attribute is a hard one to measure, but approaches have been made at the preschool level. How long and how effectively will a child work at an accepted task (that is, a contract)? The test, perhaps clinical in type, must permit, at the extremes, immediate capitulation and potent long-continued application.

The new tests must give scope to originality, style and invention. They must provide for more than a verbal medium of intellectual expression; for example, they should employ space relations, performance tasks, and scientific symbols. Theoretically the arts should be included, but this will involve practical difficulties, for it is the insights and relationships we are after and not technical skill alone. More learning-on-the-spot would appear helpful, with less emphasis on what the child already has learned and is now bringing to the test situation. Thus in a foreign language aptitude test, the ability to learn Esperanto during the testing period is an index of subsequent achievement in French and German.

The main point, however, should be emphasized. We should have *some* theory of intelligence and then construct tests consistently with it.

VI

Space does not permit me to say more than a word about nature-and-nurture in relation to intelligence. Among psychologists there is an acceptance of the idea of developmental limits along hereditary, constitutional and environmental lines. This point of view I have developed in the Thirty-Ninth Yearbook of the National Society for the Study of Education and it need not be repeated here. Present controversies are on matters of degree. When viewed in the light of test inadequacies, they are inconsequential. With everybody using blunderbusses, the game is difficult to bag!

To change one's IQ is to change one's mental status in a group. It does happen. Similarly subgroups may go up or down relatively to a larger group extant, or previously formed and measured. This too happens. One effect of improving the tests will be to increase this sensitivity to changes in the child's behavior, changes related to happenings in the organism. But as psychology is not and never can be physiology, so intelligence cannot be reduced to brain cells, brain waves, or genes. The higher processes involve the lower, but the sequence is irreversible.

Having pitched our definition of intelligence, now as in the early days of Binet and Spearman, high in the evolutionary and individual scale, we cannot climb down without losing meaningfulness. To say this is not to belittle but rather to encourage the investigation of all underlying conditions in the fields of genetics, anatomy and physiology. They have their own validity and may produce insights that can be transmuted into the currency of psychological events.

Intelligence, as a system of behavioral manifestations, is culturally determined. The intact organism, grossly similar to others, may nevertheless be 'feeble-minded.' Such depressants as physical exhaustion, illiteracy and other cultural impoverishments, together with the profound effects of taboos,

traditions and emotional blockings, may serve effectively to retard what we call normal mental growth.

With everything right and good in social dynamics, however, the feeble-minded will not die out. Organic defects and deficiencies exist; some, but certainly not all, are carried down in genetic lines which no guidance or favoring can bring to normality. Such persons are happy enough, provided that we do not force them through meaningless mental motions. Most of the ones I know are not only 'earning their keep' (in an institution), but have developed a certain repose and charm. Like the 'best people,' they tend to have too few offspring to assure ultimate survival, unless their ranks are constantly restored from the large middle population.

With a clarification of concepts and the reconstruction of measurements, the old issues will take a new form. Analysis will surely become more individualized; there will be a return to what *this child* knows and does, and what he can do under optimum conditions of stimulation and guidance. Thus may we learn to bring children to their highest levels of intellectual endeavor and social richness.

[MS. received September 23, 1940]

AN EVALUATION OF THE POSTULATES UNDERLYING THE BABCOCK DETERIORATION TEST

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There is a general tendency for clinicians and workers in the field of guidance to evaluate the efficacy of a test on pragmatic basis without inquiring as to the correctness of the theoretical postulates underlying the construction of the test. In fact many of these individuals are not even interested in finding out what psychological mechanisms the test actually measures, and those who construct tests are often guilty of the same omission, as long as it has predictive value in a larger field of endeavor. Although the practical value of a test remains unaffected (provided that its validity has been established) by either faulty reasoning as to the psychological mechanisms which are measured or lack of knowledge of these mechanisms, nevertheless, no adequate progress can be expected to be made in a field which has little knowledge of its own measuring devices. The testing program has failed to yield basic psychological principles and thus stimulate experimental work since the main purpose of test constructors has been simply to add more measuring devices to an already copious list of tests. For this reason most of these tests remain psychologically meaningless.

It is my purpose to examine the fundamental postulates underlying the Babcock test for mental deterioration and to point out that the author has made what appears to this writer fallacious assumptions as to the reason why the test measures deterioration. This statement in no way questions the validity of the test as a predictive instrument, but simply refers to the theoretical basis on which the test was constructed. Ample experimental evidence has accumulated to show that the test does measure deterioration in cases of paresis (1, 14),

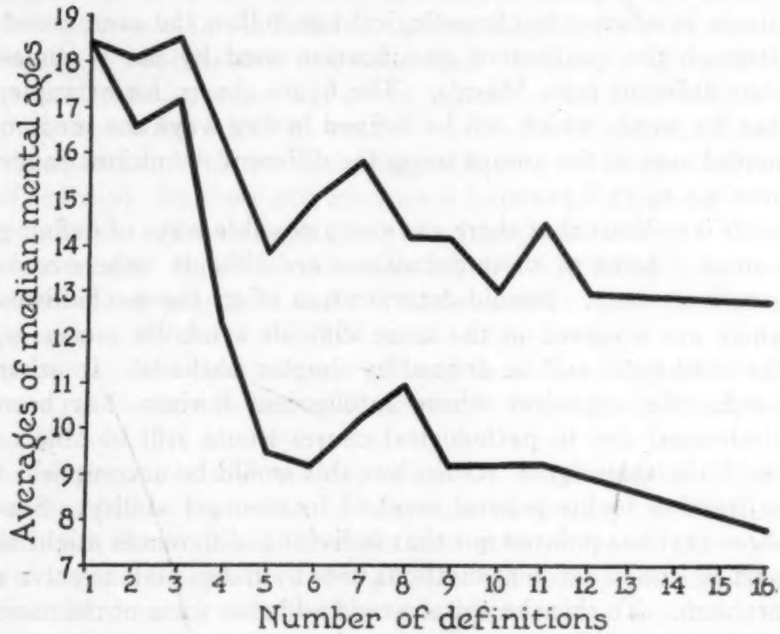
schizophrenia (2, 12), epilepsy (3, 14), and senility (6),[1] although its effectiveness has recently been questioned by Capps (4).

In approaching the problem of deterioration Babcock (2) states that the "first law of mental deterioration" is "that oldest learning is the last to be lost." This hypothesis may be accepted at its face value since there have been various clinical reports to substantiate it, although it is difficult to verify experimentally. Babcock then assumes that vocabulary is an old habit which, it follows, fails to deteriorate in proportion to the scores on tests which require recent associations. An index of deterioration is obtained by comparing the score on a vocabulary test with scores on tests 'to perceive and to fixate new data.'

The assumption that vocabulary is an old habit is not substantiated, however, by the available evidence on the development of vocabulary as a function of age, since there is evidence from vocabulary test results used in reading, scholastic achievement, and intelligence tests to show that the size of one's vocabulary develops up to maturity, and possibly, in a select group of subjects, to the age of 45 years (5). Just why this assumption is made is not at all clear since some of the items of the Babcock test which are purported to measure recent behavior, such as tracing a star shaped maze, writing the sentence, "I am going to get well very soon," or naming the days of the week or the months of the year, are present at an early developmental state in the repertoire of behavior of the organism, as is evidenced by the mental year placement of some of these items on the Stanford-Binet test. There is also good reason to believe that most abilities cease to develop beyond the age limit of maturity as is evidenced by the limits placed on the chronological ages by all intelligence tests.

[1] Altman and Shakow (*J. educ. Psychol.*, 1937, 28, 519), Davidson (*Brit. J. Med. Psychol.*, 1938, 17, 93), Dearborn (*Amer. J. Psychiat.*, 1927, 6, 725), and Simmins (*J. ment. Sci.*, 1933, 79, 704; *Brit. J. Med. Psychol.*, 1934, 14, 113; *Charac. & Person.*, 1935, 4, 25) have made use of tests, similar in theory to the Babcock scale but different in content, to measure deterioration. They report that the tests differentiate psychotics, etc., from non-deteriorated subjects, although, according to Altman and Shakow, the relationship between the degree of deterioration and the scores on such tests need not be positive.

Although one of the main assumptions made by Babcock, that vocabulary is an older habit in relation to the solutions required on many of the other items of the Babcock test, is untenable, the further problem confronts us to explain the reason why the test is valid as has been reported by most of the individuals using it. It must be true that within certain



The general population uses from 1 to 16 different definitions for each word of the first 50 words of the 1916 Stanford-Binet vocabulary. These words are grouped in terms of the number of definitions used for each word and the graph shows the averages of the median mental ages of individuals using the easiest (bottom curve) and the most difficult (top curve) definitions. (Compiled from Marx's, 11, data.)

limits vocabulary shows the least degree of deterioration as compared with other forms of behavior, and it is this fact which remains to be explained.

A clue to the explanation is offered by the work on the qualitative differences of the difficulty of definitions. It is, of course, known that a majority of words can be defined in many qualitatively different and yet appropriate ways. After analyzing the responses of subjects to the Stanford-Binet vo-

cabularies Marx (11) reports a maximum of sixteen different definitions of a word, and Green (8) a maximum of seven. Not all of these definitions are of equal difficulty. This fact is shown in the figure, using Marx's data as more appropriate for our purpose since she studied the vocabulary of the 1916 Stanford-Binet which is used by the Babcock scale. Green's results, showing the distribution of the different kinds of definitions in relation to chronological age, follow the same trend although the qualitative classification used by her is somewhat different from Marx's. The figure shows, for example, that for words which can be defined in five ways the median mental ages of the groups using the different definitions range from 9.5 to 13.8 years.

It is evident that there are many possible ways of defining a word. Some of these definitions are difficult, others comparatively easy. Should deterioration affect the mechanisms which are involved in the more difficult symbolic processes, the word could still be defined by simpler methods. In other words, the organism whose intellectual horizon has been diminished due to pathological causes would still be able to reach the same 'goal' result, but this would be accomplished in relation to his general level of intellectual ability. Seashore (13) has pointed out that individual differences might in part be due to different methods used by individuals to solve a problem. To this the writer would add that some of the more efficient but difficult work methods require a certain level of general ability and that deterioration might destroy the mechanisms which underlie the more difficult solutions. Goldstein (7) presents some very pertinent data on this question. He shows that certain brain lesions do not destroy the ability to solve a problem, but affect the *method* by which the problem can be solved. Krechevsky's (9) report of animal's 'detour' behavior lends further credence to the above hypothesis. He showed that rats with cortical lesions who are presented with two possible solutions of a maze problem are less able to use a difficult solution even though that solution is more efficient since it requires the expenditure of less energy. The present writer has used human subjects on a one alley maze which

can be solved by a number of different methods and finds that the kind of solution used is dependent on the general level of intelligence of the subject. Sherburne (work cited by Maier, 10) has obtained similar results on a reasoning test.

It becomes clear now why the vocabulary score does not deteriorate in proportion to the other behavior measured by the Babcock test. The deterioration test makes use of only the end results in the definition of words, and, therefore, superficially it appears as if the vocabulary remains unaffected by general deterioration. It seems logical to assume, however, that the reason why the vocabulary of the deteriorated individual remains relatively unchanged is because easier methods of reaching the same end results are left to the organism even if the more difficult solutions are no longer available. Most of the other items of this test require controlled behavior where the appropriate and efficient solutions can be achieved by one or at least a minimal number of different kinds of responses. As would be expected it is the scores on these latter tests which are affected to the greatest degree in deteriorated patients.[2]

If the more difficult definitions of words are no longer present in the repertoire of responses of the deteriorated individual, then one might expect that not only would there be a change in the qualitative aspects of the definitions, but also that the number of definitions available for each word would be reduced. That this latter situation is apparently true for deteriorated epileptic patients has been reported by Capps. For words which can be defined in at least five different ways he found that, "The least deteriorated group could occasionally give as many as four or five meanings for each stimulus word, but the most deteriorated group could give two meanings to a stimulus word only about three times out of twelve. This group could very rarely give as many as three meanings for a single word" (4, p. 34). Capps assumes that the explanation for these results is that the deteriorated patient

[2] It is possible that the spread of scores over a wide range of years on the Stanford-Binet test which has been considered as indicating deterioration is likewise due to the differences of the number and the difficulty of solutions which exist between the different items of the test.

is not able to shift readily from one meaning to another. It is possible, however, that the more difficult definitions are dependent on mechanisms which have been affected by the deteriorating process, so that the patient gives fewer definitions because he is able to define the word only in terms of its simplest meanings.

There is little reason to suppose that even if only the end results of the ability to define words are measured, the vocabulary scores of deteriorated patients will remain completely unaffected, since even the easiest definitions of some words are too difficult for the lower mental age groups as can be seen in the figure. Capps, in fact, found a direct relationship between the degree of deterioration and the scores on certain tasks which primarily measured the ability to use words. He reports that scores on written tests (with the exception of an antonym test) correlate to a higher degree with deterioration than the scores of tests on which the subject responded orally. An examination of the kinds of tests which he used shows that the following conditions differentiated the written and oral tests: that the correct answers to the written tests were predetermined and allowed the subject no alternative in obtaining the correct solution, and that the oral tests (homograph, free word association, and the Stanford-Binet vocabulary) could be answered by many appropriate responses. Thus the written tests gave the subject only one or at best a few methods of solving the problem whereas the oral tests could be passed by utilizing a number of different solutions. This is the same condition which obtains between the items of the Babcock test and the vocabulary score, and can, therefore, be explained on the same basis, that is to say, if a number of methods are available for reaching an end result, the more difficult solutions need not be available to the subject in order to obtain the correct response.

The verification of the hypothesis, that the reason that the vocabulary scores of deteriorated patients show a minimal amount of change from the pre-deteriorated level is because there are many appropriate solutions of varying difficulty and that the final result of defining a word can be made on the

basis of easier definitions even if the mechanisms involved in the more difficult conceptual organization are no longer available, demands two lines of research: (1) qualitative analysis of the definitions of words used by deteriorated and non-deteriorated subjects; and (2) the comparison of methods used in the solution of problems where the goal result can be reached by a number of different methods of varying difficulty.

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FEDERICO KIESOW

1858–1941

A few weeks ago the veteran of Italian psychologists, Federico Kiesow, Emeritus Professor of the Royal University of Turin, LL.D. *honoris causa* of Wittenberg College (Springfield, Ohio), died after a prolonged illness.

The deceased was born at Brüel (Mecklenburg-Schwerin) in 1858. He took his doctor's degree in philosophy in Leipzig in 1894 and was assistant, first of Wilhelm Wundt in Leipzig, and later of the great Italian physiologist, Angelo Mosso.

Professor Kiesow founded the Psychological Institute at Turin University. In this institute, which he directed as professor in ordinary from 1906 to 1933, his gifts as scientific investigator, as guide and friend of his students, found full scope. Under his enthusiastic leadership the Turin Institute acquired a world-wide reputation as a busy center of psychological research. Among his numerous publications should be mentioned those on taste sensitivity, thermic and tactile points, geometro-optical illusions, so-called spontaneous representations, the Weber-Fechner Law, the specific function of the organs of sense, feelings and their concomitant phenomena, the conceptions of sense and sensation, reaction times, eidetic phenomena, dreams, the conception of soul, psychic causality, etc. In addition to these experimental researches should be added his philosophical papers on Socrates.

Professor Kiesow, with unflagging zeal for his science, soon gathered together a select group of pupils and thus created his own school. Among his pupils may be mentioned A. Gemelli, A. Pastore, L. Agliardi, Y. Leeda, J. Civalleri, M. Ponzo, L. Chinaglia, R. Halin, A. Fontana, L. Botti, A. Yatti, V. D'Agostino, A. Costa. Of these some predeceased their master; others are now either university professors of psychology, or lecturers, or *private docents* of psychology, theoretic philosophy or of various medical branches.

A whole-hearted admirer of his great masters, Wilhelm Wundt and Angelo Mosso, Professor Kiesow always laid emphasis on their theories and researches and quoted them freely in his publications.

After a few years spent in Italy, Professor Kiesow became a naturalized Italian, blending serenely his devotion to the land of his adoption with his nostalgic love of the Germany of his youth.

For his pupils he was not only a teacher of science but a leader in life. His own long life was laborious, fatiguing and often full of suffering, but he bore all with the unshrinking courage of a fighter who asks no quarter. In every trial, in every reverse, he was for his pupils a model of serenity, and self-control. When his pupils were faced with the vicissitudes of life, he inspired them with faith and courage, and taught them to accept without flinching the discipline of duty.

His work as scientist and teacher remains a living thing and he will always be remembered by Italian psychologists as a striking figure among their most eminent pioneers.

MARIO PONZO

Royal University of Rome

A whole-hearted admirer of his great masters, Wilhelm Wundt and Angelo Mosso, Professor Kiesow always held [illegible] them in their theories and researches and quoted them freely in his publications. [illegible]

After a few years spent in Italy, Professor Kiesow became a naturalized Italian. [illegible] his devotion to the land of his adoption with [illegible] of the [illegible]

But [illegible] in life. [illegible]

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